

Time, Life and Environment: Practices of Geohistory at the
Intersection of the Earth and Life Sciences

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For my family.

Abstract

In his great work on fossil bones (1812), Georges Cuvier compared our ignorance of geohistory to our conceptual mastery of the heavens. Several centuries of research had “burst the limits of space” and drawn back the curtain on the hidden mechanism of the universe. Yet deep time remained obscure, shielded from inquiring eyes by the inconvenient fact that the past no longer exists. To “burst the limits of time,” scientists needed to overcome this barrier—needed, in other words, to extend their epistemic reach into the deepest stretches of geohistory.

This dissertation is framed by this grand epistemological challenge. *How do scientists “burst the limits of time” in order to unravel the complicated thread of time, life and environment?* Cuvier was among the first people to show how the vanished contents of deep time might be reconstructed from surviving material evidence. Yet his achievement did not solve the epistemological problem once and for all. Over the past two hundred years, scientists have burst the limits of time again and again—new barriers, new ruptures. It is this process that interests me. I am particularly interested in how scientists from multiple disciplines pool their conceptual and material resources to reconstruct different aspects of complex historical events. In addition, I am interested in the strategies researchers have developed to probe the interactions between living things and their environments on a range of spatial and temporal scales.

The dissertation is organized into five chapters of unequal length. After a brief Preface, Chapter 1 situates the project in three overlapping bodies of literature. These are: (1) philosophical studies of the “historical sciences,” (2) philosophical discussions of paleontology, and (3) philosophical discussions of scientific practice associated with the

“practice-turn.” This exercise enables me to articulate my aims for the project, and (no less important) to say what this dissertation is *not* about.

The remainder of the chapters concern historical and philosophical topics in the sciences of geohistory. Chapter 2 examines a “start-up problem” in nineteenth century geology: how were fossils turned into a reliable yardstick for measuring geological time? I argue that in order to use fossils to measure time, geologists had to overcome a “problem of nomic measurement” (so named by Hasok Chang). Moreover, and contrary to philosophical expectations, they did not do this by formulating a theoretical explanation of the operative phenomena. Instead they pursued a more piecemeal strategy guided by practices of heuristic appraisal—something I suggest is typical of justification in start-up situations.

In Chapter 3, I turn to the subject of explanation, and explore why explanations of complex historical events tend to grow more complicated over time. Using inquiry into earth’s largest mass extinction as an illustration, I argue that the main driver of explanation in geohistory is “non-explanatory work”: work that may be relevant to the evaluation of explanatory hypotheses, but that is not undertaken in the interest of testing an explanatory claim. This “drives” explanation by bringing new features of historical phenomena into focus—and this in turn creates new demands (adequacy conditions) on explanations, prompting investigators to develop more complex explanatory models.

In Chapter 4, I explore the concept of “uniformitarianism”: perhaps the most contentious term in the geological literature. Since this is a polyvalent term, many commentators have assumed that its controversial status arises from a sort of semantic chaos, which sows confusion among otherwise competent language users. However, I argue that debates about uniformitarianism in geology do not arise from a mere babel of meanings. Instead, they arise from legitimate disagreements about substantive questions, for example, “Is uniformitarianism necessary?” and “When is it appropriate to offer non-uniformitarian explanations of past events?” This chapter examines these questions, and relates them to several “forms of understanding” pursued by researchers in geohistory.

Finally, in Chapter 5, I explore the emergence of a new approach to stratigraphic complexity, first in stratigraphy, and then, following its creative appropriation, in paleobiology. The approach is based on pioneering models of sedimentary basin filling, and has come to be associated with an approach known as “stratigraphic paleobiology.” This chapter traces the emergence of stratigraphic paleobiology and explores how it reconfigured the cultural landscape of paleobiology following the Paleobiological Revolution. It also considers how the new stratigraphy is shaping paleontological discussion of “incompleteness” and “bias” in the fossil record.

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Preface:

Bursting the Limits of Time (and Again)

1. A Little History of Historical Reconstruction

In the early decades of the nineteenth century, Georges Cuvier's scientific star was on the rise. The reason was his flair for resurrection; for draping old bones in living fabric, and even divining the life habits of prehistoric beasts (Rudwick 2005). "Is not Cuvier the great poet of our era," Balzac asked in his philosophical novel, *La peau de chagrin*:

Byron has given admirable expression to certain moral conflicts, but our immortal naturalist has reconstructed past worlds from a few bleached bones; has rebuilt cities, like Cadmus, with monsters' teeth; has animated forests with all the secrets of zoology gleaned from a piece of coal; has discovered a giant population from the footprints of a mammoth. These forms stand erect, grow tall, and fill regions commensurate with their great size. (Balzac 1901, 21)

Cuvier didn't do half these things, of course. Yet his research on fossil bones was still a revelation (Dawson 2016). In a series of publications, Cuvier reconstructed complete skeletons for a range of extinct creatures, and proceeded to divine further things, like their external appearance and likely habits (Rudwick 1992). In all this, comparative anatomy held the key, and in particular, the notion of *correlation*. The idea was that only some combinations of characters are functionally viable—so animals with sharp teeth must also have stomachs capable of digesting meat, as well as other parts fitted to catching and subduing prey.¹ Because of this, one can begin with a tooth or partial skeleton, and by tracing out a string of interdependencies, arrive at an understanding of the organism as a whole:²

He can call up nothingness before you without the phrases of a charlatan. He searches a lump of gypsum, finds an impression in it, says to you, "Behold!" All at once marble takes an animal shape, the dead come to life, the history of the world is laid open before you. (Balzac 1901, 21)

Cuvier's reconstructions were especially important for the nascent venture of *geohistory*: the project of reconstructing the history of the planet. The reason is that they showed the possibilities that lay dormant in the ruins of the past, should the naturalist approach them in the right frame of mind. Just as past scientists had "burst the limits of

¹ The alternative was non-viability; an animal whose parts were not so finely adjusted could not fulfil its conditions of existence, and hence could not exist.

² Cuvier never claimed to be able to reconstruct an entire animal from a fragment of bone (as later historical mythmaking has claimed). He simply claimed that the principles of comparative anatomy enable one to "determine the class, and sometimes even the genus of the animal to which it belonged, above all if the bone belonged to the head or the limbs" (Rudwick 1997, 36).

space,” so Cuvier would “burst the limits of time,” enabling an unprecedented look at the planet’s deep history (Rudwick 2005). His accomplishment was to penetrate the mists of time to reveal the succession of increasingly alien worlds that had preceded our own. Each bizarre creature he raised from the dust inspired more confidence in this program, and added to his almost Newtonian stature among natural historians (Appel 1987; Rudwick 2014).

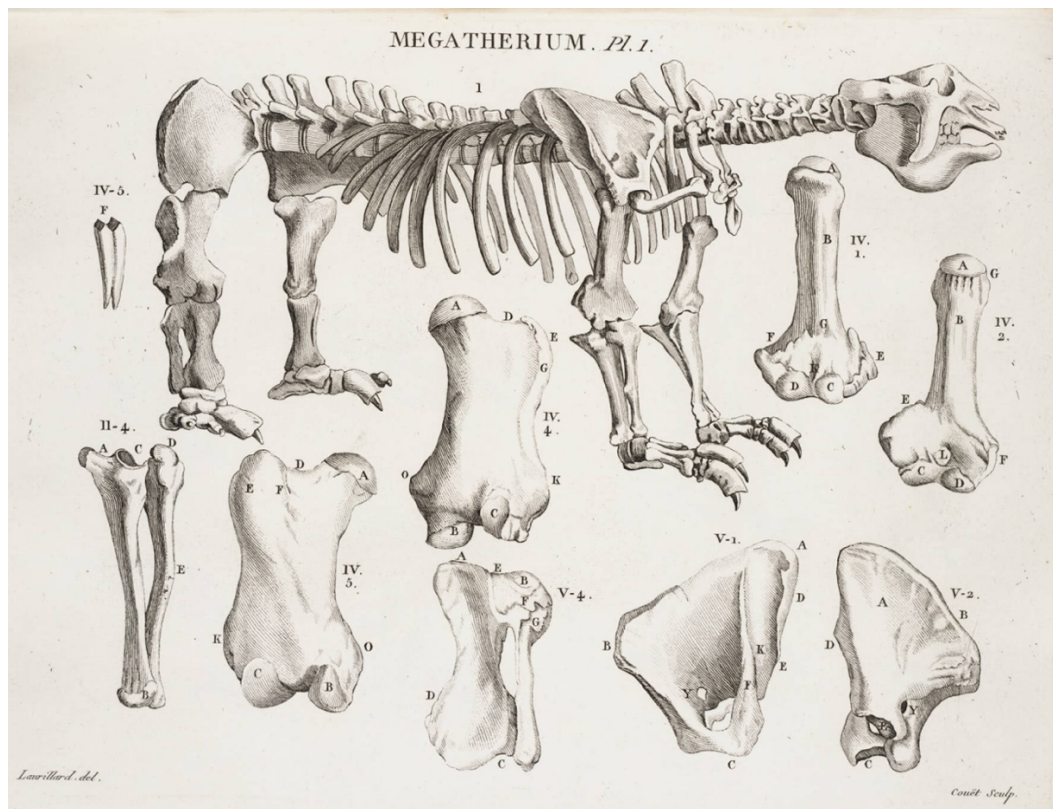


Figure 1 Georges Cuvier’s reconstruction of the giant ground sloth *Megatherium*, from his *Recherches sur les ossements fossils* (1812).

Although Cuvier's popular acclaim rested on his seemingly miraculous resurrections, no less significant was his geological study of the Paris Basin, conducted in collaboration with the mineralogist Alexandre Brongniart. It had earlier been conjectured that Paris rested at the center of a shallow bowl formed by the distinctive Chalk formation, which could be observed in outcrops around Paris but not in the city itself (Rudwick 2014, 130).³ Utilizing surface exposures, as well as underground quarries and boreholes, Cuvier and Brongniart were able to confirm this hypothesis. In addition, they pieced together the order of formations overlying the Chalk—a succession of sandstones, clays, limestones and gypsum (the burial place of many of Cuvier's resurrected creatures). Crowning their achievement was a colored map depicting the locations of the major formations surrounding Paris from a bird's eye perspective. This was not the first map of its kind, but at least before the publication of William Smith's great map in 1815, it was the most influential specimen of this increasingly important genre (Rudwick 2005).

More innovative than the map, however, was the accompanying “general and ideal section,” which pictured the pile of formations in cross-section, as if the Parisian countryside had been excavated to reveal a cliff face (Figure 2). The section was “general” because it depicted the complete succession of formations at one location. It was “ideal” because the location was imaginary—no actual outcrop revealed the pile of formations in perfect order. For ease of interpretation, the section was pictured free of post-depositional disturbance, with all its layers restored to their original, more-or-less horizontal positions. Ultimately, it served two functions. First, it neatly summarized a

³ The person responsible for this hypothesis was none other than Antoine Lavoisier, revolutionizer of chemistry and ill-fated “[Tax] Farmer General,” who was guillotined in 1794 on trumped-up charges of defrauding the state (Gould 1989a).

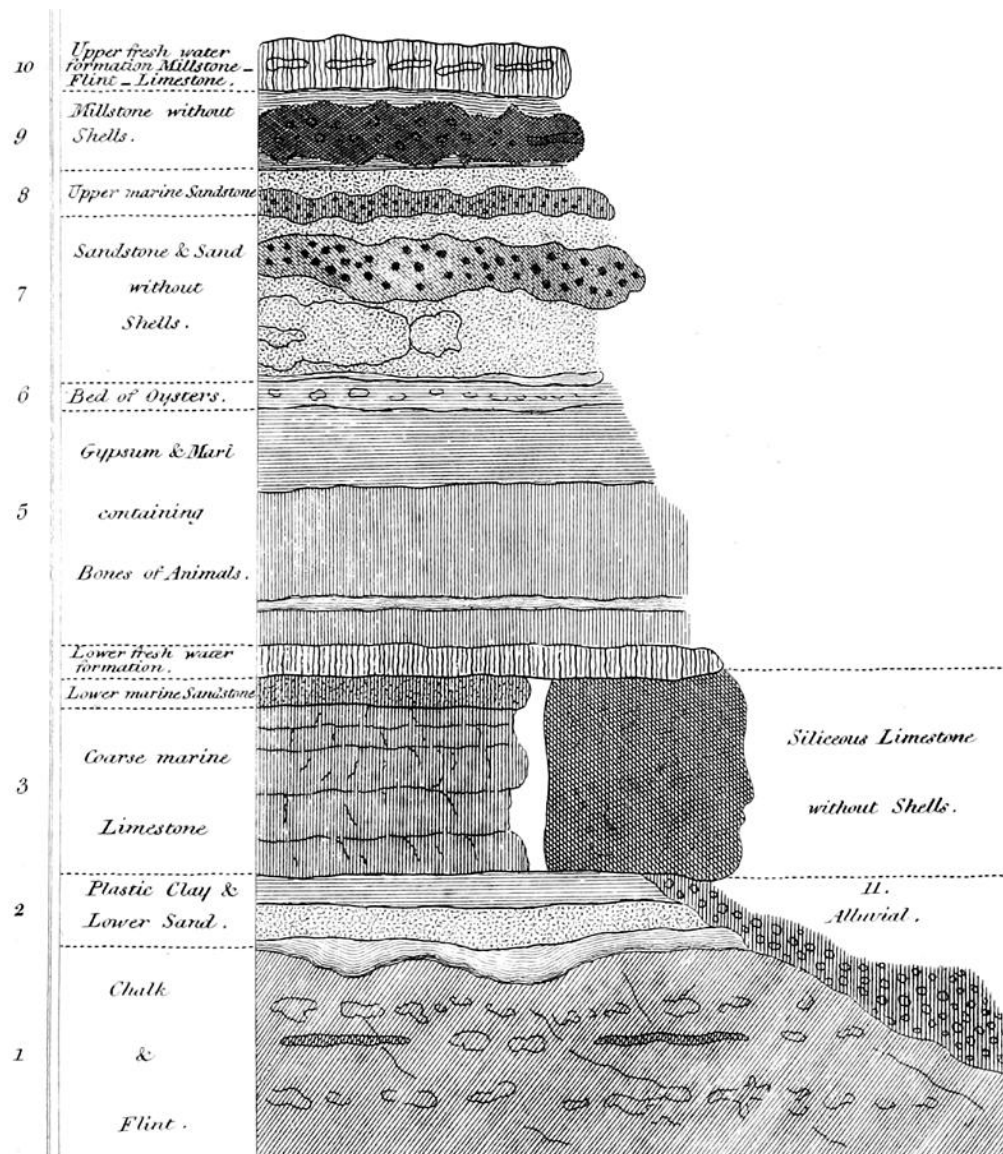


Figure 2 Georges Cuvier and Alexandre Brongniart’s “general and ideal section” for the Paris Basin, first published in 1811.

great deal of information; at a glance, researchers could gain an appreciation for the order and thickness of formations that may be poorly developed or even absent at particular locations. Second, it enabled geologists to see *into* the earth, and when used in conjunction with the bird’s eye map, to translate two dimensions into three across the

whole of the Paris basin (Rudwick 1976; Frodeman 2003). These were considerable accomplishments, and much of early nineteenth century geology was given over to their elaboration and extension (Laudan 1987; Rudwick 2008).

Geological sections remain an important part of geology's "visual language," and Cuvier and Brongniart's study would have been important if it had done no more than furnish a template for how similar diagrams could be constructed for other locations (Rudwick 1976; Rupke 1998). But in fact it did a good deal more than this. In Rudwick's words, "[the] detailed analysis of the Paris Basin...became the most influential example of how the static three-dimensional structures described by [structural geologists] could be transformed into a dynamic history of a specific part of the Earth" (Rudwick 2014, 132). The key, in this case, was fossils. In the course of their inquiry, Cuvier and Brongniart noticed that different parts of the pile contained different suites of distinctive fossils. This was important for two reasons. First, it helped with the task of stratigraphic correlation—with the identification of the same package of rock in different geographical locations.⁴ Second, and more important, it provided a means of inferring how environmental conditions had changed during the interval in which the rocks had been deposited. Cuvier and Brongniart observed, for example, that the distinctive Coarse Limestone contained fossils easily recognizable as members of well-known marine genera. This suggested that the area around Paris was once inundated with seawater, enabling its colonization by organisms adapted to saltwater conditions. Higher in the pile they found fossils resembling modern freshwater mollusks, suggesting that, at other

⁴ Stratigraphic correlation is the way geologists establish relative age relations among rock formations (see Chapter 2).

times, the Paris Basin had been a lagoon or a large lake. And then, higher up, marine fossils again. Evidently, marine and freshwater conditions had passed the baton several times over the history of the basin, with drastic (and devastating) results for the resident fauna.⁵

As noted, Cuvier and Brongniart's study had a massive influence on their contemporaries, providing "a fruitful model for further research across Europe on what soon became known as Tertiary formations" (Rudwick 2005, 557). For the remainder of the century, stratigraphy was to be the staple scientific work of most geologists, and stratigraphic correlation their central problem (see Chapter 2). A typical publication from this period consisted in "a detailed description of the formations (and their fossils, if any) in some specific area. This was usually illustrated with a geological map and often also by sections showing the pile in profile"—just the technologies Cuvier and Brongniart had pioneered in their study of the Paris Basin (Rudwick 2014, 141). Once the succession of formations had been unraveled locally, the next task was to correlate successions in distant areas to form an integrated picture—and here fossils proved especially useful. Indeed, it was largely by the use of fossils that stratigraphy achieved its fabulous success during the nineteenth century, including the assembly of a broad outline of geohistory—the geological time scale—which is still recognizable in its major features today.

But fossils are not just valuable as instruments for correlation. In addition, they are sensitive indicators of environmental conditions—at least to the extent that you can infer the environmental preferences of long-dead organisms. This is most feasible when

⁵ Later geologists challenged this interpretation, most influentially, the pioneering geologist Constant Prévost (see Rudwick 2008, Ch. 10).

fossil species have living relatives whose environmental preferences can be observed (as in Cuvier and Brongniart's study). But even for more ancient species, it is often possible to make inferences about their life habits, and to use this information to constrain inferences about past states of the earth. Most reef-building corals, for example, prefer nearshore marine environments where light penetrates all the way to the seafloor (Ager 1963). This generalization is not universal (coral gardens have recently been discovered in the deep sea, beyond the reach of sunlight). Still it is reasonable to assume that most fossil corals lived in shallow marine conditions—at least since the beginning of the Mesozoic Era, about 250 million years ago. In the earlier Paleozoic Era, different forms of coral flourished, meaning that inferences from contemporary observations are less applicable. Yet these inferences can still be made in a great many cases, as when ancient corals are found alongside other reef-builders in large limestone masses, and can even be rendered secure by calibrating actualistic expectations with non-biological proxies (see Chapter 4).

In this way, biological and geological questions intertwine: questions about the habits of past organisms and the characteristics of physical environments; questions about the spatial and temporal distribution of fossil taxa and the succession of rock formations. Still, these are only the simplest questions that can be asked at the interface of biology and the earth sciences. In recent decades, the advent of more precise geochronology and stable isotope geochemistry have given scientists the ability to probe the relationship between life and its environment in increasingly rigorous ways (Bottjer 1995; Erwin 2006a; Knoll 2011). Scales of resolution previously unthinkable have been achieved—it is now possible to resolve quarter-billion-year-old events to a period of a million years or

less, at least under special circumstances (see Chapter 3). And this, in turn, has facilitated precise comparisons between physical environmental changes and biological events like extinctions and adaptive radiations (Droser 1995; Harries 2003; Erwin and Valentine 2013; Patzkowsky 2017). At the same time, a better understanding of the processes by which sedimentary basins are filled has given paleontologists new tools for understanding the stratigraphic distribution of fossils, and for dissecting the famed incompleteness of the fossil record (Chapter 5). The old project of “reading” the fossil record has in this way achieved a new level of sophistication, largely in virtue of conceptual and technological resources imported from outside of paleontology (see Brett 1995; Holland 2000; Patzkowsky and Holland 2012).

This dissertation is about the reconstruction of geohistory: about how scientists from a range of disciplines join together to unlock the potential of the rock record. It is also about scientific learning: about the processes involved in *better* reading the rocks of the earth as records of its history. Cuvier was one of the first naturalists to demonstrate how the vanished contents of deep time might be reconstructed from surviving material evidence. Yet his achievement did not solve the epistemological problem once and for all. In the past two hundred years, scientists have burst the limits of time again and again—new barriers, new ruptures. It is this project that interests me. I am particularly interested in how scientists from multiple disciplines pool their conceptual and material resources to reconstruct different aspects of complex historical phenomena. In addition, I am interested in the strategies researchers have devised to probe the interactions between living things and their environments on a range of spatial and temporal scales.

There is more to say about the broad motivation for this project, and I will say it in Chapter 1. The remainder of this Preface provides an outline of the dissertation, with brief descriptions of individual chapters.

2. Outline of Chapters

This dissertation is organized into five chapters of unequal length. The first (“Situating the Project: Practices of Geohistory at the Intersection of the Earth and Life Sciences”) aims to situate the project within three overlapping bodies of literature. These are: (1) philosophical studies of the “historical sciences”; (2) philosophical studies of paleontology in particular; and (3) philosophical discussions of scientific practice associated with the “practice turn.” At each juncture, I identify key themes and concerns, and attempt to say what is distinctive about my topic and methodological approach. I also discuss the “time, life and environment” of my title, which I regard as the thematic “toothpick” holding together this “deli sandwich” of a dissertation. (Read on, and this metaphor will begin to make sense.)

In Chapter 2 (“Measuring Time with Fossils: A Start-Up Problem in Scientific Practice”), I turn my attention to nineteenth century geology, and explore a key challenge that geologists faced in assembling a global geological time scale. This challenge was methodological, and concerned the practice of paleontological correlation: the “matching” of rocks in different geographical areas on the basis of the fossils they enclose. Paleontological correlation was the key to integrating information from scattered outcrops, and to ordering their constituent formations into a globally consistent series or “geological time scale.” However, contrary to philosophical expectations, this practice

lacked a sound theoretical justification during the period of its most conspicuous success. This chapter examines what this lack of justification amounted to, as well as how geologists validated the tool of paleontological correlation in ongoing inquiry. In short, I argue that geologists faced a version of what Hasok Chang (2004) calls the “problem of nomic measurement,” and that they solved it by adopting a thoroughly forward-looking approach oriented around considerations of heuristic merit. (A somewhat leaner version of this chapter will appear in the December 2021 volume of *Philosophy of Science*.)

Chapter 3 (“Explaining the Apocalypse: The End-Permian Mass Extinction and the Dynamics of Explanation in Geohistory”) takes up the subject of scientific explanation. This is a perennially hot topic in philosophy of science, which has generated a large and complex literature focused on several interrelated topics. However, philosophers have exhibited a blind spot to the questions of how explanatory projects develop over time, as well as what processes are involved in generating their developmental trajectories. This chapter examines these topics using research into the end-Permian mass extinction as a case study. It takes as its jumping-off point an observation from Adrian Currie (2014, 2019a), that explanations of historical events tend to grow more complex over time. But it goes beyond this observation by scrutinizing the processes responsible for generating this pattern. Surveying several decades of research into the end-Permian extinction, I suggest that the principal “driver” of explanation in geohistory is *non-explanatory work*: work that is undertaken to increase our descriptive understanding of a phenomenon, not to test a particular explanatory claim. Non-explanatory work drives explanation by imposing or eliminating demands on explanation, and by furnishing new resources for constructing explanatory models. Explanations grow

more complex because (1) the demands on explanation tend to increase with ongoing characterization of the target phenomenon, and (2) characterization tends to grow the roster of explanatory resources. However, the fact that non-explanatory work sometimes *eliminates* demands on explanation means that this trend is not irreversible. I suggest that to achieve a more rounded view of the dynamics of explanation, philosophers must ask how research into complex phenomena is organized, as well as how explanatory progress depends upon the coordination of different kinds of material and epistemic resources. (This chapter is forthcoming in *Synthese*.)

Chapter 4 is called “Uniformitarianism Re-examined, or The Present is the Key to the Past, Except When it Isn’t (and Even Then it Kind of Is).” It examines perhaps the more contentious term in the geological lexicon: *uniformitarianism*. Since this is a polyvalent term, many commentators have assumed that its controversial status arises from a sort of semantic chaos, which sows confusion among otherwise competent language users. However, I argue that debates about uniformitarianism in geology do not arise from a mere chaos of meanings. Instead, they arise from legitimate disagreements about a range of substantive questions. This chapter examines these questions, and relates them to several “forms of understanding” pursued by researchers in geohistory (Parker 2014). These forms of understanding are: (1) understanding what happened; (2) understanding why something happened; (3) understanding complex earth systems; and (4) understanding the geological record itself. Each form of understanding, I claim, is associated with a different sense of “uniformitarian[ism]”: uniformitarian[ism] as (1) a method of reasoning; (2) a kind of explanation (or cause); (3) an assumption about earth system dynamics; and (4) a kind of study, respectively. And each of these is associated

with a different substantive question: (1) is uniformitarianism necessary; (2) when is it appropriate to give (non-) uniformitarian explanations of past events; (3) does the evolution of the earth system—including the changes associated with the Anthropocene—render uniformitarianism otiose; and (4) how useful are uniformitarian (or “actualistic”) studies of the geological record itself? This chapter examines these questions and suggests that it is because they are severally disputed that uniformitarianism continues to occupy a contentious place in geological discourse. In addition, it offers both a historical perspective and a forward-looking appraisal of the role of uniformitarianism in geological practice.

Chapter 5 (“Biased, Spasmodic and Ridiculously Incomplete: How Sequence Stratigraphy Helped to Unlock the Potential of the Fossil Record”) is a historical study. It concerns the advent of a new approach to stratigraphic complexity, first in geology, and then, following its creative appropriation, in paleobiology. The approach is associated with a set of models that together revolutionized stratigraphic geology during the 1970s and 1980s. These included the celebrated sequence stratigraphic models of Peter Vail and others, which show how the processes of sedimentary accumulation impart a complex structure to the stratigraphic record. Transposed into paleobiology, they gave researchers a powerful way of studying the incompleteness of the fossil record, and of removing biases imposed by the structure of the rock record itself. In addition, they helped to reconfigure the cultural landscape of paleobiology, giving a new impetus to fieldwork and eroding the barrier that Gould and others had erected between the “old” and the “new” paleontology. This chapter traces these developments, and explores how “stratigraphic paleobiology” has begun to transform paleontological discussions of

incompleteness and bias in the fossil record. An Appendix to Chapter 5 (beginning on page 256, after the Bibliography) contains a glossary of important stratigraphic terms.

Chapter 1:

Situating the Project: Practices of Geohistory at the Intersection of the Earth and Life Sciences

1. Wrapped in History

The earth is wrapped in its history—in layer upon layer of sandstone, shale, granite and garnet schist—like a newborn baby in a swaddle. Thin by comparison with the radius of the planet, this wrapper is nonetheless vast in extent, with stacks of rock measuring tens of kilometers in total thickness. Sedimentary rocks are the main repository of historical information about events and conditions at the earth's surface. These form when granular substances sink to the bottom of a body of water, or are washed or blown to places where they can be buried and consolidated. Igneous rocks also contain useful information, in particular about the chemical evolution of the planet. These form when molten materials cool to the point where they harden or crystalize, much like

water turning into ice. A third kind of rock forms when other rocks are subjected to high temperatures and pressures, altering them without turning them into liquids. These metamorphic rocks are not so useful as historical documents; yet like igneous rocks they contain valuable information about a variety of subsurface processes. Altogether, the geological record is a vast repository of information: information about the history of life and the evolution of earth's surface environments; information about the dynamics of earth systems and the composition and behavior of the planetary interior (Miall 2015; Knoll 2021). The challenge for scientists is to extract this information, and ultimately, to mobilize it in support of claims about unobservable events and conditions, including those in the deep past (Knoll 2003; Fortey 2004; Bjornerud 2012).

This dissertation is about how this is done, in a particular and somewhat unusual sense. Specifically, it is about how scientists engaged in reconstructing geohistory organize and equip themselves to exploit the pools of information contained in the geological record. The dissertation fits within the “turn to practice” in philosophy of science, which is to say, it treats knowledge-making practices as its primary focus (Ankeny et al. 2011; Soler et al. 2014). However, in contrast to much philosophical work—even work framed by the practice-turn—its emphasis is on the “down to earth” aspects of scientific practice; on the routine and material, as opposed to the rarefied and abstract. What activities are involved in using the rocks of the Earth as evidence for claims about its past? How do researchers assign evidential value to material evidence, and manage the risks associated with incompleteness and bias? And what is involved in better “reading the rocks” of the earth as records of its physical and biological history? A down to earth approach is suitable for these questions because they concern matters that

are *literally* down to earth: rocks and the fossils they contain. More to the point, it is appropriate because historical reconstruction is a bottom-up activity, whose approach to the earth stands in contrast to the more idealized approaches of fields like astronomy and geophysics (Greene 2009; Rudwick 2008).¹

My treatment of these questions is far from systematic. As the geologist Raymond Moore writes, the domain of geohistory “comprises all chemical, physical, and biological conditions which have existed on and in the earth, all processes which have operated to make and modify these conditions, and all events which have affected any part of the earth, including its inhabitants, during [any] time from the beginning of the planet onward to the present” (Moore 1949, 3). Frankly, this is a lot of stuff, and it seems to follow that any unitary analysis of “historical reconstruction” or “historical inference” is bound to fail. This has not always stopped philosophers from trying to give such analyses. So, for example, Patrick Forber and Eric Griffith write that “[*the*] task of historical reconstruction involves crafting a causal etiology for a specific event or set of

¹ As Mott Greene (2009) observes, approaches to the study of the earth have long been split along a fault line, with historical geologists on one side, and physicists, astronomers and geophysicists on the other. Writes Greene: “The earth of nineteenth-century astronomy and scientific cosmology was a gravitationally governed and rotating spheroid. It had no history of note other than a steady thermodynamic course from a frozen (or fiery) origin in a distant but calculable past to a fiery (or frozen) endpoint in a distant but calculable future...Viewed from this standpoint, the earth of geology was little more than the study of transient epiphenomena, well below the threshold of scientific interest” (Greene 2009, 167–168) A major accomplishment of early geology was therefore “to give importance, coherence, and meaning to a variety of materials, structures, and processes [particulars] that held virtually no interest for astronomers and physicists” (168). They did this, as Martin Rudwick emphasizes, by constructing “a new *kind* of natural science, in which the historical dimension was central and constitutive” (Rudwick 2008, 6).

events” (Forber and Griffith 2011, 2, emphasis added).² In a similar vein, the geologist–philosopher Robert Frodeman writes that “historical science *is defined by* the role that historical [i.e., narrative] explanation plays in its work” (Frodeman 2003, 964, emphasis added; see also Gould 1989b). Finally, Aviezer Tucker states that “inferences of [common] origins form *the* epistemic basis of the historical sciences” (Tucker 2020, 105, emphasis added). Examples like this could be multiplied indefinitely. They testify to the “professional bias toward generality” that Thomas Nickles diagnoses as characteristic of academic philosophy (Nickles 1987, 52).

Now, generality is all well and good where interesting generalizations can be had. But as Nickles observes, the bias towards generality is arguably responsible for many of philosophy’s woes when it comes to analyzing scientific methodology. In particular, the need to say something general has often prevented philosophers from saying much that is very interesting about scientific methods: “By imposing virtually [impossible] demands on the subject [namely, that any analysis of scientific methodology be completely general]...philosophers leave themselves with almost nothing to say...!” (Nickles 1987, 52). Nickles is here talking about the analysis of scientific methods in general, but his point also applies to discussions of method in the historical sciences. Faced with a highly heterogeneous subject matter, philosophers can either say that the historical sciences lack a singular methodology (which is true, but boring), or claim that such a methodology exists at a high level of abstraction (which is likely to be unilluminating even if it is true).

² This involves, first, identifying a temporal sequence of events or “chronology,” and second, determining “the causal links and processes linking events across time”—a “history.”

In any event, by adhering to the idea that a philosophical account must be as general as possible, they end up on the horns of a dilemma of their own making.

The way out of this dilemma is to pay more attention to the goals and interests of historical scientists, as well as to the diversity of practices grouped somewhat uncomfortably under the rubric of “historical reconstruction.” As Adrian Currie (2017, 2018) says, we cannot assume that all historical scientists are up to the same thing, or, to use the language of a later chapter, that they are all pursuing the same “form of understanding.” Indeed, I will suggest in Chapter 4 that historical scientists are interested in no less than four distinct forms of understanding, only one of which is adequately characterized by an emphasis on narrative explanation (cf. Forber and Griffith 2011; Frodeman 2003). In addition to understanding *why something happened* (the appropriate subject of narrative explanation), geohistorical scientists are interested in *what the past was like*, in *how information is stored in the geological record*, and in *how complex earth systems operate over a range of spatial and temporal scales*. Recognizing this diversity makes it unlikely that we will arrive at any one-size-fits-all analysis of historical reconstruction, even at a fairly high level of abstraction. Or as Currie puts the point: general treatments of the historical sciences are “a mug’s game” (Currie 2021).

This dissertation heeds Currie’s warning—I have no general account of historical reconstruction to offer. Instead, my goal is to address philosophical questions in contexts sufficiently constrained to permit definite answers. I have already listed some of these questions. For example, in Chapter 2, I address the question, “What activities are involved in using the rocks of the Earth as evidence for claims about its past?” by focusing on stratigraphic practice, and in particular, on the justification of paleontological

correlation in nineteenth century geology. In Chapter 4, I address the question, “What is involved in better ‘reading the rocks’ of the earth as records of its physical and biological history?” by examining situations in which uniformitarian assumptions are relaxed in response to open-ended learning. And in Chapter 5, I examine the question, “How do researchers manage the risks associated with incomplete and biased data?” by tracing the emergence of a new approach to stratigraphic complexity, first in geology and then in paleobiology. Chapter 3 is not oriented around one of these big questions; rather, it is an attempt to elaborate a “dynamics of explanation” for the sciences of geohistory. Still, it asks a question that has not received as much attention as it should have: “Why do explanations of historical events tend to grow more complicated over time?”

It will be apparent from these remarks that this dissertation is not a tightly integrated whole, as Cuvier believed the animal body to be (see the Preface). Instead, I think of it as a deli sandwich, with assorted-yet-complementary layers held together by a thematic toothpick. This “toothpick” is the *time, life and environment* of my title. Throughout the dissertation, I am interested in how scientists study the interweaving of time, life and environment on various time scales, and sometimes, exploit this interweaving for investigative purposes. Paleontology is focal for this project, since it is paleontology that deals most directly with the interaction of living things and their environments over large spatial and temporal scales. Yet no less important is stratigraphic geology, the study of layered rocks and different kinds of information they contain. Absent stratigraphy, the fossil record would be a mere cabinet of curiosities: interesting enough, but without great interest for understanding life’s history. It follows that to

understand the evolution of the earth and its inhabitants, close interdisciplinary coordination is essential. Time, life and environment will be understood together, or not at all.

Recent developments have accented the importance of interdisciplinarity for the sciences of geohistory. Most notably for my purposes, paleontologists have increasingly sought “to interpret the fossil record within the context of Earth’s dynamic planetary history,” using geochemical analyses to reveal histories of environmental change and perturbation, and linking these to the fossil record through stratigraphic correlation (Knoll 2011, 3). The result has been mounting evidence that life has been shaped by—and in turn has shaped—the physical environment; or to put this somewhat whimsically, *that life and environment are interwoven on the loom of time*. As a recent textbook states:

We cannot understand either the physical or biological history of the Earth in isolation...because the two have been tightly intertwined: the physical environment has influenced life, and life in turn has influenced the physical environment. (Stanley and Luczaj 2015, 2)

Peter Ward and Joe Kirschvink have a similar perspective:

One [conclusion of recent studies of Earth history] is that evolutionary history has been importantly affected not only by the interplay of life (competition and predation) but also by the course of the physical evolution of the Earth, its atmosphere and its oceans [which are in turn shaped by organismic activities]. (Ward and Kirschvink 2016, 345)

Finally, this, from leading pre-Cambrian paleontologist Andrew Knoll:

Both organisms and environments have changed dramatically through time, and more often than not they have changed in concert. Shifts in climate, in geography, and even

in the composition of the atmosphere and oceans have influenced the course of evolution, and biological innovations have, in turn, affected environmental history. (Knoll 2003, 5)

Linguistic trends mark this increasingly “systematic” approach to earth’s history. A *Web of Science* search for the term “earth system” retrieves two papers from 1988, neither of which conformed to the current usage of the term (Knoll 2011).³ By 2011, however, the number of citations had grown to 237 (and a comparable search of the CSA Illumina (Georef) database yields 746 hits). In 2020, the number of citations exceeded 7,500. The relevance of this trend for paleontology was nicely captured by the popular textbook, *Principles of Paleontology*, published near the beginning of the upswing: “Paleontology has become an increasingly dynamic science, with a growing interdisciplinary focus on broadly-based research questions, propelled by discoveries reported nearly every week in leading scientific journals” (Foote and Miller 2006, 287).

This dissertation aims to approach paleontology in this light—not as a well-defined discipline with clear borders and a central mission, but as a sprawling and dynamic science, increasingly entangled with other departments of the sciences (including stratigraphy). It is not a dissertation about “earth system science,” the branch of science that treats feedbacks and other interactions between the biosphere, atmosphere and other earth systems (Steffen et al. 2020). Still, in its emphasis on the entangled nature of time, life and the physical earth, it addresses themes that earth systems science also addresses (see especially Chapters 3, 4 and 5). Even Chapter 2, which concerns the methodology of measuring time with fossils, engages the theme of time, life and

³ To speak of “the earth system” is to refer to the interacting processes of the atmosphere, biosphere, cryosphere, hydrosphere and lithosphere (individually, “earth systems”).

environment, since deciphering the temporal significance of fossils requires filtering out the signature of changing environments. In short, to use fossils to date rocks, you need to know that a fossil is present in a formation because of the period of time in which that formation was deposited, not (merely) because of favorable environmental conditions. The entanglement of time, life and environment, then, has been present in paleontological practice from its first stirrings as a distinct specialism.

The remainder of this chapter serves to situate my project within several ongoing discussions in history and philosophy of science. These are: recent philosophical discussions of “historical science” (Section 2), philosophical analyses of paleontology (Section 3), and discussions of scientific practice associated with the “practice-turn” in history and philosophy of science (Section 4). The last of these sections contains my reflections on my methodological approach, such as it is. Before coming to this, however, it will be helpful to review some themes in recent philosophical work on the historical sciences. This is the task of the next two sections.

2 Philosophy and the Historical Sciences

Like many uncharted areas of philosophical inquiry, the analysis of the historical sciences began with a few narrowly defined projects before expanding to embrace new questions and approaches. I call these traditional projects the “vindicatory,” “comparative,” and “explicative” projects, respectively. The projects are not entirely distinct and have often been pursued in tandem. Still, it is useful to separate them for analytical purposes. To this end, I begin with some reflections on historical inference and the rock record.

The geological record, taken at face value, is a repository of physical and chemical patterns. Rock layers are stacked, one upon the other; mineralogical features grade and shift, often in complicated ways; and throughout the column, the nature and characteristics of emplaced fossils vary tremendously. Geochemistry adds a further dimension to the complexity: one consisting of shifting patterns of elemental and isotopic composition. All these patterns can be exploited to unravel the pathways of geohistory, and to understand how life and its environment have interacted over a range of spatial and temporal scales (Knoll 2003). Still, there is nothing *intrinsically historical* about patterns in the configuration and composition of sedimentary rocks. Instead, these patterns are historical only by interpretation—by inferences that move from patterns to conclusions via mediating warrants. “History means interpretation,” the historian E.H. Carr wrote in his little book, *What is History?* (Carr 1961, 23). And if this is true of human history, still more is it true of *geo-history*, where the historian’s “documents” are not documents at all, but are instead material objects like stones and fossils. As Robert Chapman and Allison Wylie put it:

“[Traces of the past] don’t speak.” Material evidence is inescapably an interpretive construct; what it “says” is contingent on the provisional scaffolding we bring to bear. (Chapman and Wylie 2016, 4)

Chapman and Wylie are concerned with archaeological practice, but their message is a general one: when it comes to historical science, it is interpretation all the way down. There is no “self-warranting” foundation that scientists can build their interpretations upon as a means of ensuring their objective validity. Instead, historical inference is a bootstrapping operation, which proceeds by the assembly and iterative refinement of

provisional foundations (or “scaffolds”). These foundations “are known to be tenuous,” and are routinely broken down and rebuilt when circumstances call for it. Nonetheless, they tend to function reasonably well—so well, in fact, that scientists are often willing to place considerable confidence in their conclusions, despite their provisional and interpretive nature.⁴ The past may be inaccessible to direct observation, but this does not mean it is unknowable, provided we exercise a bit of ingenuity.

This final claim is the jumping-off point for many philosophical investigations of historical reconstruction. To take a representative example, Patrick Forber and Eric Griffith (2011) observe that while “[our] epistemic access to past events is limited, often severely so,” nonetheless “some claims about prehistory enjoy strong epistemic support” (1). Adrian Currie begins his (2017) article on historical methodology on a similar note:

Our access to the past is fragmentary: geological processes like subduction ensure that mineral traces are destroyed; the probability of an organism fossilizing and its remains surfacing are miniscule; our picture of pre-Socratic philosophy is pieced together from passing mentions in incomplete texts. In the face of such destruction, some philosophers and scientists are pessimistic about uncovering past facts; perhaps history’s mysteries will remain so. And yet, historical scientists frequently produce firmly supported, well-founded, and plausible narratives. (Currie 2017, 930)⁵

Finally, here is Derek Turner, whose concern is a particular deficit that historical scientists face—their (supposed) lack of access to experimental methods:

⁴ Currie refers to this as the “unreasonable effectiveness” of trace-based reasoning (Currie 2018).

⁵ Currie repeats this trope in his (2014): “Historical scientists appear to operate under evidential scarcity. Signals from the past degrade over time, and the large-scale, complex nature of historical targets undermines experimental access. And yet historical scientists frequently provide rich, detailed and plausible hypotheses” (188).

Like all scientists, paleontologists need to subject their hypotheses and theories to rigorous empirical tests. In many areas of science, empirical testing involves experimentation: researchers carry out a series of trials while varying the initial conditions a little bit each time in order to see how those changes affect the outcomes...[But] *no one can manipulate or experiment with the past*. This simple fact creates a major methodological problem for paleontologists: how can they test hypotheses about prehistoric life without being able to experiment on the things they wish to study? (Turner 2009a, 201)

Lest the reader conclude that the historical sciences are epistemically handicapped, Turner follows with a word of encouragement:

Contrary to the impression that one might get from textbook presentations of the scientific method, it *really is* possible to carry out rigorous empirical tests without actually performing any experimental manipulations on the objects of interest...The development of these techniques for carrying out nonexperimental empirical tests is a major—and largely unsung—scientific achievement. (Turner 2009a, 201)

Never mind the non-experimental techniques that Turner mentions. The thing to notice is how his philosophical project is set up. After observing that historical scientists are in a tough spot, epistemically speaking, a typical analysis of the historical sciences shows how this is overcome: how historical scientists beat the odds in warranting substantive claims about the past. Call this the “vindicatory project” in philosophy of the historical sciences. Its aim is to reveal the methodological, or perhaps the metaphysical, basis of success in the historical sciences, and to defend them from criticisms like that of Henry

Gee (a senior editor of *Nature*, and a paleontologist), that “No science can ever be [really] historical” (Gee 1999, 8).⁶

Closely related to the vindicatory project is the “comparative project.” This works by contrasting the historical sciences (usually taken to include geology, paleontology, evolutionary biology, archaeology and cosmology) with a different category of science (usually the “experimental sciences”).⁷ The project intersects with the vindicatory project. Turner, in his (2009a), pivots from the observation that one cannot experiment on the past to the claim that the historical sciences nonetheless possess *non-experimental* resources for evaluating hypotheses. (This, you will notice, is an answer to Gee’s criticism; see fn. 6.) But comparative analyses are not always vindicatory. In other work, Turner argues that historical sciences are at an epistemic disadvantage relative to the experimental sciences, and that this places limitations on what historical scientists can ultimately achieve (Turner 2005a, 2007, 2016). The reason, again, is the impossibility of manipulating the past: a thesis Turner dubs the “asymmetry of manipulability.” While the asymmetry of manipulability does not preclude the historical sciences from rigorously evaluating empirical hypotheses, it does place them a rung below the experimental sciences on the ladder of epistemic prowess. This is because the experimental sciences

⁶ What Gee seems to mean is that historical narratives are “subjective” assertions (basically, guesses), which “can never be tested by experiment, and [are therefore] unscientific” (Gee 1999, 5). The challenge for philosophers is accordingly set: to show that a science can be both historical *and* epistemically successful.

⁷ Aviezer Tucker, in his paper “Historical science, over- and underdetermined,” contrasts the historical sciences with the *theoretical sciences*. The distinction mirrors that which authors like Cleland and Turner draw between the historical and the experimental sciences: namely, whereas the historical sciences are interested in inferring relationships between token events, the “theoretical” (like the “experimental”) sciences are interested in relationships between types (Tucker 2011, 826).

are able to actively produce new evidence, while historical scientists are stuck with those traces of the past that have chanced to survive to the present.⁸ (For a compelling response to this argument, see Currie 2018, Chapters 4 and 5.)

No doubt the leader of the comparative project is Carol Cleland, whose 2002 paper, “Methodological and epistemic differences between historical science and experimental science,” set the agenda for much subsequent discussion of historical reconstruction (see also Cleland 2001, 2011, 2013). In this paper Cleland argues that scientists engage in “two very different patterns of evidential reasoning,” one of which predominates in the historical sciences, the other in the experimental sciences (Cleland 2002, 474). The former pattern is basically abductive: researchers observe “puzzling associations” between traces and attempt to explain them by postulating common causes—historical events that, if they took place, would explain the association among the traces. The pattern in the experimental sciences, by contrast, is predictive. It seeks to confirm or disconfirm hypotheses about *types* of events by testing predictions in well-controlled laboratory settings. Cleland’s most ingenious argument is that the patterns of reasoning succeed in their respective settings by exploiting a fortuitous feature of the world, which David Lewis labels the “asymmetry of overdetermination.” This has to do with the architecture of causality in time, and while the details needn’t concern us, the

⁸ An emphasis on evidential degradation is a prominent feature of general philosophical discussions of the historical sciences. Probably this traces from Elliott Sober’s influential work on phylogenetic reconstruction (e.g., Sober 1988). As Sober and Steel say in a recent paper, “We are interested in how the natural processes connecting past to present *constrain* our ability to know about the past” (Sober and Steel 2014, 558, emphasis added).

upshot of the analysis is vindictory: each reasoning style is well-calibrated to exploit the kinds of evidence at its disposal.

A third project in the philosophy of the historical sciences is related to the previous two, but is nonetheless distinct. It is the project of characterizing the nature of evidential reasoning in the historical sciences—a kind of reasoning, you will recall, that is basically interpretive (Chapman and Wylie 2016). This might be called the “explicative project.”⁹ Cleland’s account, again, has been influential. According to her, the method of historical science involves discriminating between mutually exclusive hypotheses proposed to explain puzzling associations between traces. In what Cleland calls the “prototypical scenario,” investigators begin by observing some puzzling association between traces (Cleland 2002, 480). They then form hypotheses to explain the association—hypotheses that postulate a common cause for the traces in the form of a long-past event. Ideally, the hypotheses will be mutually exclusive (and Cleland seems to think that they usually are). This means that the task of the scientist is to decide whether the common cause of the traces is *X* or *Y*; it is not to apportion causal responsibility between *X* and *Y*. With this task in mind, the historical scientist sets to work like a detective, gathering evidence with an eye to discovering a trace “that unambiguously discriminates one hypothesis from among a set of currently available hypotheses as providing ‘the best explanation’ of the traces thus far observed” (481). These “smoking guns” cinch the case for one causal claim over another, and while they do not identify a

⁹ The comparative project involves an explicative component; Cleland, for example, has tried to characterize conflicting patterns of evidential reasoning in the historical and experimental sciences. Yet I want to identify an explicative project in addition to the comparative project, since not all projects of explication are performed in service of comparison.

hypothesis as uniquely correct, they do break the tie of underdetermination that characterizes most historical reasoning about past events. For this reason, most historical research is oriented around the search for smoking guns.

Cleland's account has met with a great deal of criticism since it was first offered. Almost everyone who has worked on the explicative project has seen fit to take a whack at it, usually as a prelude to offering their own account (e.g., Bonnin 2019; Currie 2017, 2018; Forber and Griffith 2011; Kleinhans et al. 2005; O'Malley 2016; Tucker 2011; Turner 2005a, 2007).¹⁰ These criticisms are compelling, and taken together they are quite damning.¹¹ Still, it ought to be observed that most of Cleland's critics have agreed with her *project*—agreed, in other words, that it is worthwhile to explicate the pattern of reasoning associated with “prototypical” historical investigations. Currie comes the closest to disagreeing (and in recent publications, he seems to have sworn off the project entirely). According to him, there is no “method of historical science”: “[no] *simple* model of archetypical historical methodology” (Currie 2017, 931). The historical sciences are simply “too disunified to admit of an ‘archetypical’ characterization,” and indeed, this is precisely what we should expect, since “historical scientists are nothing if not opportunistic”:

As [historical scientists] frequently lack experimental access to their targets, and sometimes face incomplete and biased evidence, [they] apply whichever methods will maximize their epistemic reach. Identifying smoking guns, drawing together

¹⁰ Ben Jeffares's influential (2008) paper is not so critical; yet even he takes issue with Cleland's implication that the historical sciences are not interested in establishing natural regularities.

¹¹ Here is a digest: historical scientists do indeed operate like detectives in many instances, but detective work involves more than a search for smoking guns (even in the somewhat special sense in which Cleland understands this term).

independent streams of evidence, and discovering dependencies between past entities are all important parts of this story. The point is this: the success of historical science is not due to some unified method, but due to a plurality of methods. (Currie 2017, 949)

Notice, however, that this is not so much a *rejection* of the explicative project as it is an *expansion* of it. Currie's main goal in his (2017) is to "argue that testing for coherency between hypotheses about past entities is a common and important pattern of reasoning in historical science" (931, emphasis added). Historical scientists do not just search for "smoking guns," or utilize convergent streams of evidence as Wylie (2011) and others have suggested (e.g., Forber and Griffith 2011; Vezér 2015). In addition, they consider how hypotheses about the past "hang together"—that is, they perform coherency tests. It is because other accounts of historical methodology fail to notice the importance of coherence testing that Currie moves to reject the notion of a singular historical methodology. But regardless of whether he is right (my suspicion is that he is), it is clear that he is here engaged in a variant of the explicative project. He just wants to expand the stable of historical methodology beyond a single stall—he does not want to burn it down.

And why should he? It is a perfectly good stable, after all. Each of the methods philosophers have analyzed is a legitimate way of warranting hypotheses as reliable, and of adjudicating conflicting claims about the past. Such contributions are valuable, not least because the historical sciences were long neglected by philosophers alongside other sciences that lack powerful explanatory theories (Love 2014). Showing that the historical sciences possess reliable methods thus contributes substantially to their philosophical vindication. (Notably, Currie's early efforts to explicate historical methodology were in service of an overarching "optimism" about the epistemic prospects of the historical

sciences: a kind of vindictory aim.) Still, one might wonder whether the historical sciences really require *more* vindication. Nearly two decades of work has been undertaken in the name of rehabilitating the historical sciences, much of it broadly successful (Dresow 2019a). Arguably this has removed the initial motivation for pursuing not only the vindictory project, but the comparative project as well. If we agree that the historical sciences exist on a par with the non-historical sciences—or at least that they harbor no major epistemic handicaps—then it is hard to see the point of further comparisons. This is doubly the case if we acknowledge, with Currie, that the historical sciences are every bit as messy, disunified and pluralistic as the rest of the sciences (a claim that suggests that meaningful comparisons are likely to be elusive).

For similar reasons, the explicative project is in need of a rethink. In the past, this project has concerned itself with the “methodology” of the historical sciences: “how do historical scientists go about generating knowledge, and how does this work?” (Currie 2018, 137). A common presumption is that there *is* such a method—a more-or-less unitary way that historical scientists go about supporting and adjudicating claims about the past. But if this presumption is wrong—if the historical sciences lack a unitary method, even at a high level of abstraction—then attention must shift elsewhere: to more circumscribed patterns of reasoning (e.g., Currie 2015, 2017, 2018; Bokulich 2018, 2019), or else to the skilled practices involved in reconstructing the past (e.g., Wylie 2015; 2019; Wylie 2016). Instead of characterizing historical reasoning as a means of promoting epistemic optimism (methodological vindication), philosophers should interrogate how scientists assemble an understanding of the past through an iterative and deeply social process, “in the absence of infallible foundations” (Chang 2004, 234). That

is, they should investigate the work involved in historical reconstruction, and the way this makes possible the kinds of inferences upon which the enterprise of geohistory depends. (For cognate proposals, see Tamborini 2019 and Currie 2021.)

Chapman and Wylie (2016) provide an example of what the project looks like in action. Their study of evidential reasoning in archaeology is premised on “the conviction that considerable wisdom is embodied in the creativity and skilled practice of archaeologists” (Chapman and Wylie 2016, 6). In order to make this wisdom explicit, the authors target “key instances of exemplary practice, critical turning points, innovations, and instructive failures in the use of archaeological data as evidence” (Chapman and Wylie 2015, 7). In other words, they perform a case-based analysis of evidential and material practices in archaeology, aimed at isolating “the norms of evidential reasoning that have taken shape in the context of...practical experience working with archaeological [materials].” Such norms are poorly captured by “idealized accounts of ‘scientific’ [reasoning],” they claim, since these were assembled in response to internal philosophical debates about issues like underdetermination and scientific realism (Chapman and Wylie 2016, 7). Instead, analyses of scientific reasoning are illuminating to the extent that they are made responsive to the details of practice in a particular domain: to how scientists actually build, analyze, integrate and adjudicate evidential arguments. This is the heart of the “turn to practice” in philosophy of science—a topic I will return to in Section 4.

In the next section, I take up the subject of philosophical treatments of paleontology. It is not a review of everything that has been written in this area. Happily, the philosophical literature on paleontology is now large enough that any such review would be a heavy lift. (For a dated but more comprehensive literature review, see Turner

2014.) Rather, the section identifies some themes and tropes in the philosophical literature on paleontology, and suggests a way forward. It also makes contact with the small philosophical literature on stratigraphy, thus identifying the crucial interdisciplinary nexus that later chapters will explore.

3. Paleontology in Philosophical Focus

Philosophical interest in paleontology is a relatively recent development. Elizabeth Lloyd was perhaps the first philosopher to devote considerable time to conceptual issues in paleontology. Her pioneering work on species selection and “emergent fitness”—some of it coauthored with Stephen Jay Gould—remains an important contribution to an ongoing debate in paleontology and evolutionary theory (see, e.g., Lloyd 1988; Lloyd and Gould 1993; Gould and Lloyd 1999).¹² Likewise, Todd Grantham’s “Hierarchical Approaches to Macroevolution,” published in the *Annual Review of Ecology and Systematics*, remains a useful exploration of some theoretical issues in macroevolution (Grantham 1995). Subsequently, Grantham explored the role of fossils in phylogenetic reconstruction and the epistemology of “taxic paleobiology” (see below), making him arguably the first philosopher to specialize on philosophical issues in paleontology (Grantham 1999, 2004, 2009). He was joined in the early 2000s by Derek Turner, who contributed a number of important studies, first on the reconstruction of animals from fossil remains, and then on the reconstruction of evolutionary trends and the

¹² A somewhat more difficult work to locate is *Interactions*, co-authored by the paleontologist Niles Eldredge and the philosopher Marjorie Grene (Eldredge and Grene 1992). Certainly this is not a book about paleontology (it is about the evolution of social systems), but it incorporates a paleontological perspective in its emphasis on hierarchical structures and multi-level causation.

role of contingency in macroevolution (Turner 2000, 2005b, 2009b, 2010, 2011; Inkpen and Turner 2012). And the number of philosophers interested in paleontology has only grown in recent years.¹³

It is customary in the philosophical literature on paleontology to distinguish two projects in which paleontologists are centrally involved.¹⁴ The first is the reconstruction of organisms from fossil remains—an activity Cuvier called “resurrection in miniature” (see the Preface), and which today goes by the name of *phenotypic reconstruction*.¹⁵ The second is the reconstruction of evolutionary patterns based on large datasets, in particular, counts of taxonomic appearances and disappearances in the fossil record. This has no generally accepted name, although the approach that uses large datasets to characterize evolutionary patterns in the fossil record is called “taxic” because it involves counting the taxa found at particular times and places (Eldredge 1979; Grantham 2009). I will call it *macroevolutionary reconstruction*, to distinguish it from the narrower project of reconstructing the evolutionary history of individual lineages or groups of lineages (*evolutionary reconstruction*).¹⁶ Evolutionary reconstructions are similar to what the philosopher Brett Calcott has called “lineage explanations” (Calcott 2009).

¹³ Especially noteworthy are the contributions of Adrian Currie, Alan Love, Joyce Havstad, Leonard Finkelman, John Huss and Alisa Bokulich.

¹⁴ The source of this convention seems to be Turner, who writes in his (2009) that paleontology includes “at least two very different kinds of research” (202). He reiterates this in his *Paleontology: A Philosophical Introduction*, in a section titled “Organismic vs. evolutionary paleontology” (Turner 2011, Ch. 1).

¹⁵ According to Turner (2009a), this research aims to answer such questions as: “what did [the organisms] eat, and what ate them? Were they solitary or sociable? In what sort of environment did they live? How did they die? How did they reproduce?” (202; see also Currie 2015, 189).

¹⁶ Currie (2015) regards this latter project as part of the investigative project of phenotypic reconstruction, which he takes to include two broad questions. The *phenotype*

The decomposition of paleontology into two (or three) main research areas reflects a narrowness of focus in philosophical studies of paleontology. For the most part, philosophers have been content to dwell on the biological parts of the field to the exclusion of its more geological parts; that is, they have been content to study paleo-*biology* (in particular, evolutionary paleobiology) as opposed to paleontology *sensu lato*. This constriction in focus is not without motivation. In recent decades, paleobiology has become a thriving research area, and important work in the history of science has highlighted the conceptual and institutional importance of the “Paleobiological Revolution” of the 1970s (Sepkoski and Ruse 2009; Baron 2011; Sepkoski 2012, 2013, 2017).¹⁷ Still, the constriction in focus has been so severe—and for the most part, so uniform—that it risks going unnoticed (Love 2011). This is doubly the case when philosophers make no effort to distinguish between paleontology and paleobiology, as for example Turner’s *Paleontology: A Philosophical Introduction* does not. This is a book about evolutionary paleobiology—the “new paleontology,” as Turner calls it in his (2014, 495). But never is its focus on evolutionary paleobiology philosophically motivated. Instead, Turner draws “a rough distinction between two kinds of research that paleontologists do”—phenotypic and macroevolutionary reconstruction, in my

question asks what the organism looked like, what it ate and how it was viable (cf. Turner 2009a, 202). The *evolution question* asks “which historical forces are to blame for that phenotype’s evolution” (189). Although Currie’s emphasis in his (2015) is on phenotypic reconstruction, he nonetheless observes that reconstructing past organisms “is not the only, or even the central, business of paleobiology. More important is study [*sic*] of large-scale macroevolutionary patterns revealed in the fossil record [macroevolutionary reconstruction]” (189).

¹⁷ The Paleobiological Revolution was a “campaign to upgrade the status of paleontology...by introducing a quantitative, theoretical agenda capable of producing independent contributions to evolutionary theory” (Sepkoski 2017, 53).

terminology—and then declares that he will focus on the latter (Turner 2011, 2). The reader could be forgiven for concluding that this is the main business of modern paleontology, and indeed Currie says as much in his (2015).

But even a reader that abstains from such judgments is likely to be misled by Turner's division of the field into organismal and evolutionary components (Turner 2011, 2). This is because in addition to phenotypic and (macro-) evolutionary reconstruction, paleontologists are involved in a variety of other projects, from reconstructing past climates, environments, and ecological networks, to reconstructing past sea and gas levels, plant and animal distributions, continental and landscape configurations, and large-scale biogeochemical processes and trends. Many paleontologists are also involved in reconstructing patterns of evolutionary descent—an activity known as *phylogenetic reconstruction* (or sometimes *phylogeny reconstruction*). Unlike the other projects I have listed, the reconstruction of evolutionary relationships has received extensive attention from philosophers, and several studies have even examined the role of fossil and stratigraphic data in phylogenetic inference (Sober 1988, 2008; Grantham 1999, 2004). Still, for the most part, philosophers have been interested in adjudicating competing methods of constructing phylogenies, without worrying too much about where the data come from (see Velasco 2013).

Kinds of historical reconstruction—a partial list

1. Phenotypic reconstruction (the reconstruction of past phenotypes)
2. Phylogenetic reconstruction (the reconstruction of evolutionary relationships)

3. Evolutionary reconstruction (the reconstruction of the history of a taxon, or group of taxa, in relation to a set of environmental factors)
4. Macroevo­lutionary reconstruction (the reconstruction of evolutionary patterns and trends at large spatial and temporal scales)
5. Macroecological reconstruction (the reconstruction of ecological patterns and trends at large spatial and temporal scales)
6. Paleoecological reconstruction (the reconstruction of past ecological communities, patterns of distribution and abundance, niche characteristics, etc.)¹⁸
7. Paleoenvironmental reconstruction A: depositional conditions (the reconstruction of past depositional environments)
8. Paleoenvironmental reconstruction B: earth systems (the reconstruction of past states of the oceans and atmosphere, as well as the past operation of earth's geochemical cycles)
9. Paleobiogeographic reconstruction (the reconstruction of past distributions and movements of plants and animals)

I do not want to give the impression that philosophical studies of paleontology have been exclusively concerned with two (or three) types of historical reconstruction. This would

¹⁸ An older sense of “paleoecology,” still in circulation, holds that paleoecology is “the study of past environments that contribute to applied problems and theory in the geological sciences [i.e., the physical earth sciences], particularly facies analysis and the reconstruction of past environments” (Kitchell 1985, 91 fn). Here I am using “paleoecology” in a somewhat newer sense; the older sense would view the primary activity of paleoecology as (7).

be particularly unfair to Adrian Currie, who in his recent work has begun to investigate some cases of truly *geo*-historical reconstruction in paleontology and geology (Currie 2017, 2018, 2019). Instead, what I want to suggest is that philosophical studies of paleontology have only recently and tentatively moved beyond a near-exclusive preoccupation with the “new paleontology,” evolutionary paleobiology. And this constriction of focus has had certain undesirable effects, in particular, a tendency to ignore more geological aspects of the science (staples of the “old” field or stratigraphic paleontology).

The tendency to ignore stratigraphy is particularly egregious in light of the central role that stratigraphic frameworks play in much paleontological, including paleobiological, research.¹⁹ As a popular textbook in paleobiology puts it, rock stratigraphies provide “the essential framework that geologists and particularly paleontologists use to accurately locate fossil collections in both [time] and [space]...A stratigraphy, illustrated on a map and in measured sections, is required to monitor biological and geological changes through time and thus underpins the whole basis of Earth history” (Benton and Harper 2009, 23, 25). Erwin and Valentine, writing from the standpoint of Neoproterozoic and Cambrian paleobiology, have a similar take:

The development of [a] high-resolution geologic timescale [for the Ediacaran and Cambrian Periods] and the correlation of geologic sections from every continent through biostratigraphy, chemostratigraphy and magnetostratigraphy have been *essential to any detailed study of rates of geological and evolutionary change*. With

¹⁹ To be clear: I am not claiming that stratigraphy is a *part* of paleontology (although the research project called “biostratigraphy” most certainly is). I am only claiming that stratigraphy is critical for much paleontological research, and that to understand paleontology, one must understand its intersection with stratigraphy.

the framework in hand we can now calculate such rates and, for example, determine how closely various evolutionary events correlate with changes in the physical environment that may have been causally related. (Erwin and Valentine 2013, 28)

Finally, here is Shen et al. (2011), writing about the paleobiology of mass extinctions:

“Detailed time scales for extinctions and recoveries are essential for understanding the physical, ecological, and chemical changes and for testing possible causes” (Shen et al. 2011, 1367). In short, paleontologists both use and contribute to stratigraphic frameworks in order to (1) fix the order of events in the rock record, (2) aid in the determination of rates of change, and (3) facilitate the integration of biological and geological information over a range of spatial and temporal scales.

It is therefore noteworthy that “stratigraphy” is absent from the indexes of both book-length treatments of paleontology written by philosophers (Turner 2011; Currie 2018). Likewise few articles in philosophical journals deal with stratigraphic concepts or practices in any detail.²⁰ (This is beginning to change, largely in virtue of the pioneering work of Alisa Bokulich, e.g., 2019, 2020.) I can think of two reasons why this may be the case, both of which may be partly to blame in this case. The first has to do with stratigraphy’s historically less-than-stellar reputation among the geosciences. The second has to do with how the historiography of paleobiology interacted with early work in the philosophy of paleontology.

Begin with the former. As the stratigrapher Andrew Miall admits, stratigraphy “has a reputation as a dull descriptive science” (Miall 2015, 271). During most of the

²⁰ Two early exceptions are David Kitts’s *The Structure of Geology*, which deals with geological notions of time in the 7th chapter, and Robert Frodeman’s *Geo-Logic*, which deals with certain aspects of stratigraphic practice in the 6th chapter.

twentieth century, an education in stratigraphy consisted in “lists of formation names and detailed descriptions of lithologies and fossil content, with little in the way of enlightenment about what it all meant.” The stratigraphic paleobiologist Steven Holland echoes this assessment:

If your undergraduate experience in stratigraphy was anything like mine, it was overwhelmingly dull. It was all about nomenclature and classification and endless lists of formation names. There were apparently no questions to be asked and the goal of stratigraphy was to devise boxes into which to place all kinds of geologic data. (Holland 1999, 409)²¹

More humorously, the stratigrapher P.D. Krynine is reported to have said, in 1941, that “[stratigraphy] can be defined as the complete triumph of terminology over facts and common sense” (Folk and Ferm 1966). All this seems to suggest that stratigraphy is a science unworthy of philosophical attention. It is all drudgery and nomenclature—an area bereft of interesting questions and laden with baroque terms like “turbidite” and

²¹ No one has discussed the excesses of geological terminology with more wit than John McPhee. Recalling his own geological education, McPhee writes: “I used to sit in class and listen to the terms come floating down the room like paper airplanes. Geology was called a descriptive science, and with its pitted outwash plains and drowned rivers, its hanging tributaries and starved coastlines, it was nothing if not descriptive...There seemed, indeed, to be a little of the humanities in the subject. Geologists communicated in English; and they could name things in a manner that sent shivers through the bones. They had roof pedants in their discordant batholiths, mosaic conglomerates in desert pavement. There was ultrabasic, deep-ocean, mottled green-and-black rock—or serpentine...There were festooned crossbeds and limestone sinks, pillow lavas and petrified trees, incised meanders and defeated streams...Someone developed enough effrontery to call a piece of our earth an epieugeosyncline...All of that...was difficult enough for the layman to remember before the diffractometers and the spectrometers and the electron probes came along to present their multiplex cavils” (McPhee 1998, 31–34).

“epeirogeny.” Given philosophers’ preferences for big questions and bold explanatory theories, is it any wonder that stratigraphy has gone mostly unnoticed?

Yet this line of thinking is misguided for several reasons. To begin, stratigraphy was the centerpiece of nineteenth century geology—a fashionable and absurdly successful science that is responsible for no less a wonder than the geological time scale (O’Connor 2007; Rudwick 2014; see also Chapter 2). To dismiss it because it is supposedly boring or burdened with strange terms is philistinism of the worst kind.²² But even accepting that philosophers should not bother with “dull descriptive science[s],” this label does not attach to the current field of stratigraphic geology, for as Holland writes: “stratigraphy [in recent decades] has undergone a *conceptual revolution*” (Holland 1999, 409, emphasis added). The occasion for this revolution was the development of new models of stratigraphic architecture (so-called “sequence models”), as well as the advent of new methods of correlation, like high-resolution event stratigraphy (see Chapter 5). Together, these have comprehensively reconfigured research in stratigraphy, with ramifying consequences for other sciences, including paleobiology. As Miall observes:

The modern science of stratigraphy...operates by the dynamic interplay between an array of deductive models and hypotheses which express our understanding of earth system science in the sedimentary realm. Most of these are qualitative, in the sense that they depend on descriptive data, but all are characterized by rigour in the protocols for field data collection, description and processing. (Miall 2015, 277–278)

²² For a reflection on the importance of descriptive science from a scientific perspective, see Grimaldi and Engel (2007). For a historical reflection on the treatment of descriptive science, with special reference to the metaphor of “stamp collecting,” see Johnson (2007).

So much for stratigraphy's bad reputation. The other explanation for why philosophers have neglected stratigraphy has to do with the prominence of evolutionary paleobiology in recent philosophical discussions of paleontology. This phenomenon dates from about 2009, when David Sepkoski and Michael Ruse edited a collection of essays called *The Paleobiological Revolution: Essays on the Growth of Modern Paleobiology*.²³ This was followed in 2012 by Sepkoski's monograph, *Rereading the Fossil Record*, which bears the subtitle *The Growth of Paleobiology as an Evolutionary Discipline*. Sandwiched between these publications was Turner's *Paleontology: A Philosophical Introduction*, which styles itself "a (mostly) non-partisan guide, with a strong philosophical slant, to some of the big ideas and questions about evolution that came out of the paleobiological revolution" (Turner 2011, 11). Together, these publications served to consolidate early philosophical interest in paleontology around a small number of topics, as well as a particular conception of paleobiology as an evolutionary discipline.

What does this have to do with stratigraphy? Here the details of the history (and the historiography) matter. During the middle decades of the twentieth century, a number of American paleontologists grew dissatisfied with what they saw as the subordination of paleontology to stratigraphic geology (e.g., Knight 1947; Newell and Colbert 1948). Paleontology was a separate science, they insisted, and a *biological science* at that. Still, it remained the case that "the majority of teachers of paleontology...[were] stratigraphers or petroleum geologists, concerned entirely with the application of paleontology to geology" (Newell 1948/9, quoted in Sepkoski 2012, 58). As a consequence, little

²³ This collection contained a number of essays by philosophers, for example, Grantham (2009), Huss (2009), Laubichler and Niklas (2009), Ruse (2009) and Turner (2009a).

progress was being made towards an understanding of fundamental biological questions arising out of the fossil record.²⁴ J. Brookes Knight, an invertebrate paleontologist, put the point tartly in his 1947 presidential address to the Paleontological Society of America:

[What] we today call a paleontologist, particularly that jellylike variety without a backbone, incapable of standing erect on his own two feet, the invertebrate paleontologist, is not a paleontologist at all. He is a geologist, a stratigraphical or “soft-rock” geologist. He has considerable familiarity with invertebrate fossils, to be sure, but he is a geologist nevertheless. (Knight 1947, 284)

This sentiment, and the rhetoric of subordination, would continue to find voice in paleontology into the 1960s and 1970s. Invertebrate paleontologists were insufficiently biological, the criticism ran, and for this reason had been excluded from important discussions, including discussions of evolutionary theory (Sepkoski and Ruse 2009; Sepkoski 2012). Gould (1970) went so far as to complain that invertebrates had been treated as if they lacked a *history* (by which he meant a history of directional change; see Dresow 2017). As such, when he and others hoisted the revolutionary flag in the 1970s, it was natural that they should fold the rhetoric of subordination into their campaign to upgrade the status of paleontology within evolutionary theory. Here, for example, is Gould, writing in the main organ of the Paleobiological Revolution, *Paleobiology*:

Invertebrate paleontology has cast its institutional allegiance with geology—more by historical accident than by current logic. When it operates as a geological discipline,

²⁴ The quoted passage is from a memo that Norman Newell (an invertebrate paleontologist) distributed to his colleagues at the American Museum of Natural History in “1948 or 1949” (Sepkoski 2012, 57).

paleontology has tended to be an empirical tool for stratigraphic ordering and environmental reconstruction. As a service industry [of academic and economic geology], its practitioners have been schooled as minutely detailed, but restricted experts in the niceties of taxonomy for particular groups in particular times. (Gould 1980, 98)

This theme has been amply reproduced in the secondary literature on the Paleobiological Revolution. Turner, for example, attempts to capture the spirit of the Paleobiological Revolution in a series of seven slogans, the first of which is “Paleontology has more to contribute to biology than to geology.” Sepkoski expresses a similar sentiment in *Rereading the Fossil Record*:

The dilemma faced by paleontologists in establishing paleontology as a legitimate *evolutionary discipline*...involved both asserting the theoretical value of the fossil record and repositioning paleontology within the larger matrix of *evolutionary biology*...Instead of being an “idiographic” field concerned mostly with digging up, describing, and cataloguing individual fossils, paleontology would now focus on large-scale quantitative analyses of patterns in the history of life...The image of the paleontologist would change, too. Gone was the picture of a dusty fieldworker who spent his life absorbing the minutiae of a single group of extinct organisms. The new model paleontologist was trained in biology as well as geology, was adept at quantitative analysis, was prepared to employ general theoretical models *to explain how evolution worked*, and might be more comfortable seated at a computer than at a fossil preparation table. (Sepkoski 2012, 2–3, emphases added)

All this is quite right, of course. Still, there is a danger we might misinterpret statements like these, and read into the Paleobiological Revolution lessons that history does not teach. One such lesson is that paleontology (or at least the “new” paleontology, evolutionary paleobiology) is essentially a bioscience, and not a geoscience at all. No philosopher to my knowledge has made so blunt a claim. Yet Turner comes close in his (2011); and since his work was foundational for the philosophy of paleontology, it is not

implausible that this perspective helped to set the trajectory of the field. Turner, at least, recommended this trajectory in his (2014). A good “starting point” for philosophy of paleontology, he suggests, “is to focus on the ways in which scientists like Gould drew upon philosophy, and in particular on the distinction between idiographic and nomothetic science, to help clarify the goals of the new paleontology” (Turner 2014, 495). Patterns of analytical attention suggest that this suggestion was heeded; much early philosophy of paleontology was philosophy of paleo-*biology*, and much of this was framed by the interests of Gould and others. But as I have argued in this chapter, paleontology is not just paleobiology by another name, nor is it a science that has been rendered obsolete by the advent of paleobiology. Instead, it is a thriving and diverse science, much of it oriented around biological questions, yet still very much a member of the earth sciences (see Chapter 5).

The upshot is that philosophers cannot approach paleobiology as if it were just another biological discipline, or a branch of evolutionary biology. Paleontologists are indeed biologists, but they are geologists too, and increasingly geochemists. That is, they are *hybrids*, and the hybrid nature of paleontological research demands philosophical scrutiny in its own right. How do geosciences marshal their collective resources to interdisciplinarily reconstruct geohistory? What roles does paleontology play in these projects, and how does it rely on other disciplines, like stratigraphy? And how do the demands of geohistorical research structure patterns of multidisciplinary collaboration and exchange? These questions have only rarely been asked, and when they have been asked, answers have tended to focus mostly on the borrowing of explanatory resources (see, e.g., Currie 2015, 2018). This dissertation will examine them from a different angle:

the angle of investigative practice (see especially Chapters 3 and 5). I explore what this means in the next section.

4. Philosophy and Scientific Practice

The final task of this chapter is to say something about my methodology. This is not an easy task, since philosophers tend to be remarkably unreflective about their methods, and I am no exception (Daly 2010). To make matters worse, my chapters range from standard philosophical analyses to more discursive treatments to historical investigations. This diversity precludes a conventional statement of methodology; yet it will nonetheless be useful to locate my project within some broad methodological currents in philosophy of science. Further, since I have criticized certain extant philosophical treatments of the historical sciences on methodological grounds, it will be useful to attempt a statement of what sets my approach apart.

As I noted in Section 1, philosophers have a well-known bias towards generality (Nickles 1987). The reasons for this are many and mutually reinforcing, but an important one is the centrality of *abstraction* as a mode of reasoning within the discipline (Love 2008a; Chang 2011). Abstraction consists in the exclusion of details from an inquiry for the purpose of facilitating understanding over different degrees of exclusion. There are many reasons for employing it. Traditionally, philosophers have abstracted to “get behind appearances”—to see past the frills and ornaments of the world in order to discern things in their real aspect and relation (Horsten and Leitgeb 2009). More recently, philosophers have abstracted as a means of model-building: facilitating comprehension by drawing attention to particular features of complex situations (Williamson 2018). Philosophers of

science “abstract from...the actual practice of science to reconstruct patterns of scientific activity...analyzing or criticizing [them] in terms of how well [they] serve the ends of the scientist” (Wimsatt 1974, 673). In all these cases, the use of abstraction is motivated by the belief that details obscure what is philosophically relevant about a concept or situation. The devil may live in the details, but the good God is in heaven, and faced with the choice, most philosophers prefer the heaven of ideas to the shifting contingencies of the material world.

There is a tendency to think of abstraction and generalization as coupled: as if abstraction is only valuable as a means of extending the scope of a claim, and generality only achievable by way of abstraction (Love 2008a). But in fact the two concepts come apart (Chang 2011). There are ways of achieving generality that do not rely on abstraction, and (more important for our purposes) uses of abstraction that do not aim to generalize. Most philosophical research employs abstraction in some form, but the kind and degree of generality sought differ widely between research projects. My project aspires to only a modest degree of generality—I am not interested in how historical reasoning works in general, nor in how the historical sciences as a whole investigate the past. My concern is rather to explicate a particular collection of practices involved in reconstructing and explaining the past. To do this, I will make use of abstraction as a means of characterizing “significant patterns of scientific activity”; but my goal is not to generalize—it is to *complexify*. Scientific research is immensely complex, and there are limits to what can be learned from highly abstract models, constructed in response to internal philosophical debates. A challenge for philosophers is to make sense of this

complexity without abstracting it away; to dissect complexity, as opposed to retreating to scales of resolution where it loses its urgency and its capacity to puzzle.

This approach fits broadly within a recent movement in philosophy of science, variously termed the “practice-turn” and the “turn to practice” (Ankeny et al. 2011; Soler et al. 2014; Potochnik 2017). This is the second major “turn” in the past sixty years of philosophical research into the sciences, and depending on how you look at it, is either a continuation of, or a reaction against, the “historical turn” of the 1960s. In the historical turn, it was argued that a more careful look at the history of science would uncover problems and complexities not evident from a perusal of contemporary scientific publications (Bird 2008; Barker and Kitcher 2014). This made it a kind of practice-turn, since advocates of historically-based philosophy of science urged that attention be paid to what scientists actually said and did, as opposed to sanitized presentations of scientific achievements (see especially Toulmin 1953; Kuhn 1962; Laudan 1977). Yet it can also be regarded as a preamble to the practice-turn, since historically-minded philosophers fixated on a small number of topics (like theory change and scientific rationality), and thus neglected vast areas of scientific practice (like forms of experimentation not guided by high-level theoretical claims). Early institutional forms of integrated history and philosophy of science (HPS) were built on this restriction (Schickore 2011; Laudan and Laudan 2016). Indeed, it was only in virtue of this highly circumscribed agenda that HPS was able to gain an institutional toehold—one that began to crumble away as its agenda broadened (Baigrie 1994; Dresow 2020).

This broadening began in earnest in the 1980s, when a group of philosophers led by Ian Hacking took a closer look at the procedures and apparatus used to produce

phenomena in the laboratory (Hacking 1983; Franklin 1986; Galison 1987; Gooding 1990). According to Andrea Woody, this was “[arguably] the first work in philosophy of science to advocate explicitly and self-consciously for a turn to practice,” and while it was indebted to the “historical turn” of the 1960s, it also had its own set of motivating concerns (Woody 2014, 124). Chief among them was the desire to dethrone theories from their leading place in historical and philosophical studies of the sciences. Theories were all very well, of course; but in their single-minded focus on theories, philosophers had neglected other fine things, like experiments and apparatus. Experimental work has “a life of its own,” independent of high-level theory (Hacking 1983); but this life is glimpsed only darkly through the glass of theory-centric approaches. The solution lay in shifting attention from thinking to doing, and from representing to intervening. So influential was this proposal that, in later years, the practice-turn has appeared to some to be little more than a rebellion against so-called “theory bias” in philosophy of science (Soler et al. 2014).

But is this true, and if not, what does the practice-turn in philosophy of science really amount to? Despite the importance of “practice” in contemporary philosophical discussions of the sciences, this question has received surprisingly little detailed philosophical attention. According to Ankeny et al. (2011), the core of the practice-turn consists in seeing science as a process, and of re-framing traditional debates in philosophy of science in terms of “[the] activities...associated with and required for the generation of knowledge” (305). According to Waters (2014), it consists in “broadening the scope of philosophical attention to investigation, and hence towards analyzing how the integration of practical know-how, concrete knowledge, investigative strategies and

theoretical knowledge provides the basis for systematic investigation of the biological world” (121). But perhaps the most interesting answer belongs to Andrea Woody, as given in her contribution to a volume exploring the practice-turn in science studies (Soler et al. 2014). In this chapter, Woody analyzes the turn to practice as “a cluster of related, and potentially interdependent changes, *each consisting of a retreat from a particular sort of abstraction*” (Woody 2014, 123, emphasis added). The first is a turn from “conception to representation”—that is, from a focus on theories as “abstract conceptual objects characterized predominantly by their logical structure” to a focus on the activity of representing using models and diagrams. The second is a turn from the “*a priori* to [the] empirical”—from analyses couched in terms of “syntactic and semantic conditions generated through *a priori* analysis” to “accounts based on the examination of the reasoning invoked by scientists in particular contexts.” The third is a shift from conceptualizing scientists as “ideal agents to human practitioners,” which embodies an increased sensitivity to the importance of tacit or embodied knowledge, orienting assumptions (including background knowledge and laden theory) and the cognitive limitations of human agents. Finally, Woody notes a mounting interest in the social nature of scientific knowledge, which she limns as a “shift from the perspective of the individual scientist to that of particular disciplinary communities and their interactions.”

The inherently social nature of contemporary scientific practice introduces a variety of issues surrounding how knowledge is structured and transmitted, how knowledge is intertwined with issues of authority, expertise, trust, and divisions of labor, and how [the social structure of science] is established and perpetuated to coordinate community deliberation and action. (Woody 2014, 124)

Thus, for Woody, the turn to practice is a turn away from analytical tendencies associated with an interest in the ideal structure of scientific knowledge, and towards tendencies associated with a broader array of interests in scientific knowledge, its production and its use. That is, it is a turn *within the epistemological study of science* from highly abstract and general accounts (focused on the structure and validation of scientific knowledge), to more local, contextualized and “naturalistic” accounts (focused on the acquisition, transmission, representation and adjudication of knowledge). The common denominator here is knowledge, as well as an epistemological orientation that places scientific reasoning and its products at the center of the philosophical universe. It is in virtue of this orientation that practice-based philosophy of science maintains continuity with earlier projects in the field, even as its interests shift to more local topics, and its methods to more contextually sensitive modes of analysis.

There is another tradition in practice-based philosophy of science: one that consists less in “a retreat from particular forms of abstraction,” and more in a turning away from traditional epistemological concepts and problems (see, e.g., Rouse 2002; Waters 2004, 2019; Love 2008b; Wimsatt 2008).²⁵ This is a shift that trades in the customary philosophical focus on scientific reasoning for an analysis of science as a process involving collaboration, coordination and multiform scaffolding (in the form of goals, standards, institutional forms, etc.). In other words, it is a shift from an *epistemological* to a *processual* view of science; or if you prefer, from an epistemology framed in terms of knowledge to one framed in terms of investigation (or “practice”).

²⁵ Ankeny et al. mention “truth, fact, belief, certainty, observation, explanation, justification, [and] evidence” as concepts that can be “usefully re-framed in terms of activities” (Ankeny et al. 2011, 305). It is concepts like this that I have chiefly in mind.

Leonelli (2016) speaks of “a broader perspective on scientific epistemology that emphasizes its processual and embodied nature and seeks to understand science by studying the practices and instruments through which research is carried out” (69). This is more or less what I have in mind. And while some would withhold the title “epistemology” from such a perspective, it is nonetheless concerned with the conditions for knowledge acquisition and transmission, and in this sense, at least, is epistemological.

This dissertation fits better in the second tradition than the first. So, while I am concerned with scientific knowledge and its acquisition, my approach does not consist in asking traditional epistemological questions in new ways, or in reframing epistemological problems in terms of cognitive agents and their “epistemic activities” (Ankeny et al. 2011; Chang 2011). Rather, I am concerned to make sense of scientific investigation in particular, local contexts: that is, to understand how scientists work and why their investigations go the way they do. So, when I discuss justification in Chapter 2, my goal is not to provide a new perspective on scientific justification; instead, it is to better understand how nineteenth geologists got on with their work in the absence of the kind of justification that philosophers expect to see in successful sciences. Similarly, in discussing scientific explanation (Chapter 3), my goal is not to say how historical scientists explain past events; rather, it is to understand how communities of scientists construct and assess explanatory models, and to characterize the kinds of work that contribute to ongoing explanatory projects. Finally, in Chapter 4, which concerns the controversy surrounding uniformitarianism, I eschew the old question of whether uniformitarianism is conceptually coherent. Instead, I try to understand what is at stake in ongoing discussions of uniformitarianism in geology as a means of understanding why

the topic remains an object of contention. All these projects are epistemological, I contend, but only in the broad sense that trades in a focus on traditional epistemological topics for a more dynamic understanding of the scientific process.

My orientation is also thoroughly, and unapologetically, historical; and in this sense, I straddle the two methodological “turns” in recent philosophy of science. Chapter 2 concerns a methodological problem in nineteenth century geology; Chapter 3 develops a case study spanning over fifty years of research into the end-Permian mass extinction; Chapter 4 uses historical analysis to make sense of the controversy surrounding uniformitarianism in geology; and Chapter 5 is a historical study of stratigraphic paleobiology. It is an irony that most philosophical studies of the historical sciences have taken an ahistorical approach (an irony that is not lost on Currie, who notices his own ahistorical approach in *Rock, Bone and Ruin*). Of course, there is nothing wrong with analyzing historical science in an ahistorical way, but given how rich the history of historical reconstruction *is*, there is great scope for more historical modes of analysis. If there is anything methodologically distinctive about this dissertation, it is the mingling of these historical modes of analysis with analytical resources from the philosophical tradition. Indeed, this dissertation is a good example of a “local integration” of history and philosophy of science of the sort I advocated in my (2020).

5. Forward

I began this chapter with rocks. Let me end with them as well.

Throughout history, rocks have functioned as a paragon of muteness. Stones are “in some sort...better than tribunes,” Shakespeare’s *Titus Andronicus* declares, “For that

they will not intercept my tale...A stone is silent, and offendeth not, But tribunes with their tongues doom men to death” (Titus Andronicus, Act III, Scene 1). Stones, unlike tribunes, tell no tales.

Shakespeare was writing before the birth of professional geology, so perhaps he can be forgiven for perpetuating this trope. But as Marcia Bjornerud (2012) observes, stones speak clearly to those who listen. Or perhaps we should say: to those who have acquired the capacity to hear. The next chapter explores how nineteenth century geologists gained the ability to hear messages recorded in stone, as well as an important methodological challenge they faced along the way.

Chapter 2:

Measuring Time with Fossils: A Start-Up Problem in Scientific Practice

1. Assembling a Geological Time Scale

The geological time scale is “a layer cake of odd names,” many of them established in a burst of amazingly fruitful research during the first half of the nineteenth century (Gould 1987a, 76). Historian Mott Greene describes it as “a triumph of intellectual attention to singularity unequalled in the history of human thought” (2009, 171). Others have called it “the tool ‘par excellence’ of the geological trade” (Gradstein 2012, 1), and “an invaluable tool for geoscientists investigating virtually any aspect of Earth’s development, anywhere on the planet” (Walker et al. 2013, 259). Less sentimental types have called the scale “a residue of nineteenth century geology” (Erwin and Valentine 2013, 13), or else “a rickety old contraption, held together by nineteenth-

century rules and current European formality” (Ward and Kirschvink 2016, 12). Yet while some may enjoy a pot-shot at this icon of their field, geologists of all stripes share a profound admiration for the scale and what it represents: nothing less than a synthesis of the geological history of our planet—a *geohistory*.

By the end of the eighteenth century, it was widely appreciated that the earth was ancient—far more ancient than the few thousand years accorded to it by modern creationists (Rudwick 2005). But scientists remained without a strategy for ordering its far-flung “pages” and “chapters” (rock bodies) into a coherent story (a geohistory). In this, they faced a situation “not unlike the hypothetical...dilemma that historians would face if they knew that modern cultures had antecedents...but did not know whether Cheops preceded Chartres or, indeed, whether any culture, however old and different, might not still survive in some uncharted region” (Gould 1987a, 76–77). While it was simple to infer that rocks near the bottom of the pile were older than those near the top (at least in local areas undisturbed by tectonic activity), geologists lacked a reliable way of comparing the ages of widely separated rocks and of ordering these into a coherent sequence. This meant there was no way of saying whether a stack of rocks in Pembrokeshire was as old as a stack of rocks in the Appalachians, notwithstanding that they might resemble one another in superficial appearance.

All this had changed by the middle of the nineteenth century. In an explosion of “conceptual innovation [and] empirical expansion,” the newly christened science of geology had burst from the gates, and set to work disclosing the long and eventful history of the planet (Rudwick 1985, 3). In less than fifty years, a multinational community of researchers had ordered the pile of formations into a concatenation of systems, “defined

by the ever-changing history of life, and recorded by a set of names accepted and used in the same way from New York to Moscow” (Gould 1987a, 77). Remarkably, the major features of this history are still recognizable today, at least for the largest divisions of the geological column (see Figure 3). Yet no less remarkably, the practice most responsible for this success—the measurement of time using fossils—lacked an adequate theoretical foundation during the early decades of the nineteenth century. It is this observation that supplies the focus for the present chapter. In particular, I will ask how the absence of a theoretical justification caused no real disruption in stratigraphic geology during the first half of the nineteenth century. My answer will be that geologists managed to solve the “problem of nomic measurement,” so named by Hasok Chang (2004)—or if they did not solve it, at least they found a way of lessening its sting.¹ The solution was nowhere explicitly formulated, yet it was widely understood that ongoing research had rendered the foundations of paleontological correlation increasingly secure. This chapter aims to explore the logical basis of this (largely implicit) judgment.

The remainder of the chapter is organized as follows. In Section 2, I introduce Richard Boyd’s notion of a start-up problem, and suggest that the subject of this chapter can be characterized as a “start-up problem in scientific practice.” In Section 3, I provide a crash course in nineteenth century geology, which is followed, in Section 4, by a discussion of the problem of justifying fossil-based measurement. In Section 5, I consider how this problem was overcome in practice: by a piecemeal strategy, as opposed to a

¹ The problem of nomic measurement arises when researchers want to measure an unobservable quantity X based on an observable quantity Y , but the relationship between X and Y is insufficiently characterized. I will discuss the problem further in section 4.

theoretical fix-all. I conclude in Section 6 with a synopsis and a brief reflection on the features of justification in start-up situations.

Geological Scale of Time.

	Periods.	Systems.	Life.
10	Cænozoic.	Pleistocene.	Man.
9		Pleiocene.	Placental Mammals.
8		Meiocene.	
7		Eocene.	
6	Mesozoic.	Cretaceous.	Marsupial Mammals.
5		Oolitic.	
4		Triassic.	
3	Palæozoic.	Permian.	Reptiles.
2		Carboniferous.	
1		Devonian.	
0		Siluro-Cambrian.	Land Plants. Fishes. Monomy. Echinod. Pterop. Heterop. Dimy. Gasterop. Annel. Polyzoa. Zooph. Brach. Crust.

Figure 3 A nineteenth century representation of the geological time scale in customary tabular format (see Rupke 1998). Notice that many of the system names are the same as those in use today. (From Phillips 1860)

2. A Start-Up Problem in Scientific Practice

This chapter addresses what might be called a “start-up problem in scientific practice.” I owe the term “start-up problem” to Richard Boyd (1992), who speaks of “*the* start-up problem [in philosophy of science]” as the problem of explaining “the first emergence of approximately true theories within a research tradition, and thus the emergence of the reliable methods they determine [i.e., justify]” (139, emphasis added). The start-up problem is a problem, Boyd thinks, because scientific methods are deeply theory-dependent, and as a consequence, it is not an option to explain the emergence of successful scientific theories by appealing to the methods they make possible. In addition, it is not an option to explain their emergence by reference to a more basic theory-independent methodology because no such methodology exists. The upshot, Boyd thinks, is that “the emergence of epistemically successful scientific methods must have depended on the logically, epistemically, and historically contingent emergence of a relevantly true theoretical tradition rather than vice versa.” Or, to render this as a motto: No epistemically successful scientific method without a pre-existing theoretical justification.

The start-up problem I deal with in this chapter is not the same as Boyd’s start-up problem, for the important reason that it is not concerned with “the emergence of an approximately true scientific theory.” Instead, it is concerned with the emergence of a methodological practice *in the absence of a justifying theory*, and indeed, in the absence of much interest in providing such a theory. The practice is paleontological correlation and consists in the “fitting together” of rock layers in different parts of the world based on their fossil contents. It is important because, prior to the second half of the twentieth century, it was the best way for geologists to compile information from individual

outcrops into regional frameworks, and ultimately, to synthesize these into a global time scale. As Roland Goldring puts it: “Until outcrops...are correlated by time lines, there is no way of gaining any real appreciation of the temporal distribution of past environments across an area or within adjacent basins and ranges; let alone of clarifying what was going on at distant points on the globe” (Goldring 1991, 156). This means that absent a reliable means of correlating rocks over long distances, the project of reconstructing geohistory is scarcely possible at all.

But why were fossils so important for stratigraphic correlation? To answer this question, we must familiarize ourselves with some features of stratigraphic geology in the nineteenth century. The next section provides a crash course in nineteenth century stratigraphy, which will position us, in Section 4, to explore our start-up problem in scientific practice.

3. A Crash Course in Nineteenth Century Stratigraphy

Stratigraphy is the study of layered rocks (“strata”), but on a more elementary level, it is all about time (Torrens 2002). Stratigraphers are interested in determining the ages of rocks, and in using this information to delineate a sequence of geological units that can be recognized throughout a region, and even throughout the world. This involves, first, delineating packages of strata that represent discrete units of time, and second, fitting these packages together through a process called correlation. Correlation refers to the practice of matching geological units found in different localities, or to be more precise, of establishing a correspondence between geographically separated parts of a single geological unit. Sometimes called “temporal correlation,” it is the way geologists

seek to establish the time-equivalence of rock layers, and by this means, to build a framework applicable over a maximally wide geographical extent.² The trick is to show that rocks observed in different exposures are actually the same age. Rocks do not come time-stamped, after all, and since geological evidence is notoriously jumbled and fragmentary, considerable difficulties confront the project of assembling a time scale from the scattered windows afforded by natural and artificial exposures.

These difficulties were acutely felt by those nineteenth century geologists who set about unraveling local sequences and matching them with sequences in other parts of the world. The basic problem was the absence of a criterion for matching time-equivalent rocks in widely separated areas. Rock type, or *mineralogy*, had once appeared a promising criterion. According to the famous theory of German scientist Abraham Werner, all rocks on the earth's surface had precipitated from a universal ocean in order of their densities (Berry 1987). So granites, having the highest density, precipitated at the earliest period, and less dense rocks like sandstones and limestones precipitated later. Had this posit been correct, temporal correlation would have been a straightforward affair, since all that would have been required to locate a rock in the pile of formations would have been information about its mineralogical characteristics. Yet Werner's system was untenable, as observations of intrusive granite sufficed to show.³ This did *not* discredit mineralogy as a guide to delineating rocks representing discrete units of time,

² There are also non-temporal forms of correlation, but these are not my concern here.

³ Intrusive rocks are formed when liquid magma penetrates existing rock; so the existence of intrusive granite indicates that less-dense rocks can be deposited *before* granite, contrary to Werner's account.

but it did suggest that rock type alone could not supply a “measuring rod of history”—a means of placing rocks in their right temporal sequence (Gould 1987a, 81).

Enter fossils. Around the turn of the nineteenth century, the surveyor William Smith had shown that fossils can be used to distinguish a number of discrete formations in England and Wales. The most famous result of this survey was a map that depicted the succession of British Secondary strata at an unprecedented level of detail.⁴ Smith produced his map “by collecting fossils from particular localities and strata, precisely noting their geographical and stratigraphical placement, and identifying analogous strata in other locations by finding similar fossils” (Sepkoski 2017, 62). He called the fossils peculiar to a stratum “characteristic fossils.” Together they functioned as a kind of stratigraphic reference system, since finding a characteristic fossil told you that the surrounding rock belonged to *this* part of the pile as opposed to *that* part. Although Smith was not terribly concerned with reconstructing geohistory (his concerns were rather structural than geohistorical), his method was quickly adopted by those with more geohistorical interests (Rudwick 1996). A famous example is Sir Roderick Impey Murchison, who declared in 1839 that “the *zoological* contents of rocks, when coupled with their *order of superposition*, are the only criteria of their *age*” (9, emphases in original).

Smith’s work came close to supplying a paradigm for stratigraphic geology in the sense of a model of exemplary practice. In the years following his publication, no geologist could eschew the task of collecting fossils from stratigraphic sections, or at the

⁴ In the eighteenth and nineteenth centuries, the term “Secondaries” (or “Secondary rocks”) referred to a collection of well-stratified and fossil-rich limestone and shales (e.g.) that rested atop the more structurally complex “Primaries” (see Rudwick 1985).

very least describing them in his notebook. Yet Smith's accomplishment did not quite rise to the status of an exemplar in the Kuhnian sense (Rudwick 1985). This is evident from the fact that, in the early decades of the nineteenth century, doubts persisted about the priority of fossil evidence in stratigraphic correlation. At issue was precisely the matter that Smith regarded as settled: the reliability of fossils as markers of stratigraphic position. The matter was unsettled because—contrary to Smith's claim to have uncovered a "Law of Strata"—Smith had in fact discovered no law that could underwrite the extension of his method to other parts of the world, or indeed to other parts of the pile. What Smith had discovered was that fossils could be used with great reliability to distinguish a large number of Secondary formations, and that these identifications could be used to correlate rocks across England and Wales (Rudwick 2005). But it remained open to question whether the existence of certain fossils in a rock reflected the period of time in which that rock was formed (as Smith's method of characteristic fossils required), or whether it sometimes reflected something else, like the presence of certain conditions at the era of "fossil potting." The problem was a serious one, and it was clear to many that it would need to be sorted out before long-distance paleontological correlations could be regarded as anything more than provisional.

4. The Problem with Paleontological Correlation, Characterized

Here is the basic issue. By the 1830s, no one denied that fossils had a role to play in stratigraphic correlation. Yet there remained a question as to what exactly this role should be, particularly when geologists ventured beyond the relatively well-behaved Secondary formations of Great Britain and continental Europe. The question was

important since the use of fossils in correlation had both empirical and theoretical vulnerabilities. On the empirical side, what was missing was a demonstration that fossil assemblages had indeed succeeded one another in an orderly way in time, not only at a single location, but everywhere in the world these fossils assemblages happened to occur. Absent this demonstration, it would not be possible to infer the age of a rock from the identity of its enclosed fossils, since fossils that occur throughout the column carry no temporal signature. However, in the early decades of the nineteenth century, knowledge of the temporal ranges of fossils remained highly fragmentary and almost necessarily parochial.⁵ This meant that the use of fossils in correlation rested on substantial empirical assumptions, which many in the early century regarded as unwarranted, if not downright implausible (Rudwick 1985).

On the theoretical side, what was missing was an explanation of *why* the stratigraphic record is amenable to paleontological correlation. Perhaps it could not be shown on empirical grounds that the history of life consists in a linear succession of mostly discrete floras and faunas. Still, if it could be shown that this succession is expected on theoretical grounds, then the absence of an empirical demonstration could be blunted. And by the 1830s, several proposals to this effect had been proposed. On the continent, Georges Cuvier had articulated a theory of revolutions, which held that massive calamities in earth's past had served to establish divisions between successive periods in the history of life (Rudwick 2005; Sepkoski 2021). Later, Élie de Beaumont

⁵ This is *not* to say that geologists lacked evidence that the history of life was broadly directional. It was suspected, for example, that fossils like ammonites were confined to Secondary strata, and that mammals were confined to Tertiary strata. What they lacked was detailed information about the spatial and temporal ranges of (most) fossil taxa; and this raised the possibility that apparent trends in the fossil record were just that.

proposed a similar theory, which held that major periods in geological history were terminated by “epochs of elevation” associated with marine and terrestrial extinctions. These theories enjoyed considerable popularity for a time—at least before the 1840s, when de Beaumont effectively recanted. Still, they were far from universally accepted, especially in Great Britain, where the most famous revolution was a bloodless one, and political history after Cromwell was rather less tumultuous than it was in France.

A related theoretical idea was that the earth was slowly cooling from an incandescent state (Rudwick 2008).⁶ Because it was believed that organic life must have a constant relationship to the state of the earth’s surface, it seemed to follow that the community of living things must have changed in order to keep pace with the state of the earth. Advocates of this view did not interpret these changes in evolutionary terms; rather, they tended to imagine a trickle of extinctions followed occasionally by new creations, or else migrations from different climate zones. Yet even apart from this, the view was based on a false premise. The earth is not slowly cooling from an incandescent state, and the drama of life’s relationship with climate is significantly more complicated than the directionalist theories of the nineteenth century could comprehend.

Without an empirical demonstration that the fossil record is suitable for correlation, or a theoretical argument that the record can be trusted in the absence of such a demonstration, geologists faced the following dilemma. In order to use the fossil record to correlate strata over large distances, it must be the case that fossil assemblages succeeded one another in an orderly way in time throughout the sampling area. However,

⁶ This idea was shared among advocates of geological catastrophes and (some of) their opponents. For the former, it supplied a plausible mechanism for transient disruptions of the earth’s surface (Rudwick 2005).

to determine whether fossil assemblages succeeded one another in this way, some method is needed to determine whether a succession in one part of the world—e.g., a sequence showing the transition from fauna *A* to fauna *B*—is contemporaneous with a succession in another part of the world (which also shows the transition from *A* to *B*). *But this is what fossils are called upon to do*—in particular, fossils belonging to faunas *A* and *B*. The result is a circularity. Since the practice of correlation presupposes that the transition from *A* to *B* happened at the same time over the relevant area, it cannot establish that this was the case—something T.H. Huxley pointed out in an 1862 address to the Geological Society of London:

For anything that geology or paleontology are able to show to the contrary, a Devonian fauna and flora in the British Islands may have been contemporaneous with Silurian life in North America, and with a Carboniferous fauna and flora in Africa. Geographical provinces and zones may have been as distinctly marked in the Palaeozoic epoch as at present, and those seemingly sudden appearances of new genera and species, which we ascribe to new creation, may be simple results of migration. (Huxley 1862, xvi)

To mark the absence of “any method by which the absolute synchronism of two strata can be demonstrated,” Huxley coined the term *homotaxis*, meaning similarity of arrangement (of fossil successions at distinct locations). His point was that paleontological correlation could not establish that fossils succeeded one another in a regular way *in time*. All it could establish is that fossils occur in a regular vertical order *in strata*. This, in a word, was the problem with paleontological correlation during the nineteenth century.

The problem can be characterized as an instance of what Hasok Chang (2004) calls the “problem of nomic measurement.” This is a generic epistemic problem in start-up situations, and it has the following structure:

- (i) We want to measure quantity X ;
- (ii) [But] quantity X is not directly observable, [so] we infer it from another quantity Y , which is directly observable.
- (iii) For this inference we need a law that expresses X as a function of Y .
- (iv) But the form of this function f cannot be discovered or tested empirically, because that would involve knowing the values of both Y and X , but X is the unknown variable that we are trying to measure. (Chang 2004, 59)

In the present case, X is time (i.e., the age of a stratum), Y is faunal composition and f is the form of the relationship between time and faunal composition over a specified area. Early nineteenth century geologists tended to assume that observed fossil successions reflect temporal successions, not just at a single location, but at many locations separated by hundreds or even thousands of kilometers. But this was just an assumption, and as Huxley said: “It may be so; it may be otherwise.” The reason is that fossils measure time only with the assistance of an empirical assumption: that the fossil record preserves a worldwide directional signal, and that certain events recorded at widely separated exposures were effectively synchronous. And this assumption cannot be decisively validated on the strength of fossil evidence alone.

Nonetheless, it was verified, at least to the satisfaction of most geologists. The next section considers how this was done. In particular, it examines the kinds of evidence relevant to assessing the temporal significance of homotaxial patterns, as well as the judgments involved in establishing the time-equivalence of stratigraphic events.

5. Validating Paleontological Correlation

It is a remarkable fact about nineteenth century geology that geologists were aware of the problem with paleontological correlation and yet seemed to be little bothered by it. Yes, there were doubts—not only about particular paleontological correlations, but also about the tendency to assign fossil evidence priority in correlational practice (Rudwick 1985). But the dominant note in the period was one of optimism and confidence regarding the promise of fossil-based correlation. Indeed, by the time Huxley coined the term “homotaxis” in the 1860s, the tendency to award fossil evidence the right of way in stratigraphic practice had been widely accepted for more than a decade.

Were these geologists behaving rashly? Did they overreach in thinking that a geological time scale could be articulated and refined using fossil data alone? In this section, I will suggest that the answer to these questions is “no.” Nineteenth century geologists had good reason to think that the succession of fossil assemblages in strata reflected a real historical succession, at least when the appropriate cross-checks had been performed. Moreover, they had reason to think that certain events in the rock record, at least, were approximately synchronous over broad geographical areas.

Consider a sequence of three fossil assemblages (A , B and C) with suspected non-overlapping ranges in time.⁷ How can the geologist know whether the observed succession of faunas ($A > B > C$) reflects a true temporal sequence as opposed to a sequence of laterally arranged depositional environments, say? To begin, if it is true that the assemblages succeeded one another in the hypothesized temporal order, then it should

⁷ The line of reasoning pursued in this paragraph is unchanged if A , B and C name taxa (e.g., individual species or genera) as opposed to assemblages (see, e.g., Harper 1980).

never be the case that *C* appears beneath *B* at an exposure, or that *C* or *B* appears beneath *A* (Harper 1980). Likewise, it should *never* be the case that these supposedly sequential assemblages appear together in a single stratum (*A* with *B*, *B* with *C*, etc.). Observing any of these forbidden sequences or associations at any exposure is sufficient to disprove the hypothesis that *A*, *B* and *C* form a non-overlapping temporal sequence. (Sufficient, that is, if no plausible explanation of the anomaly exists, such as the inversion of a whole succession of strata or the reworking of sediments following deposition.) And while the situation is more complicated if we hypothesize that *A*, *B* and *C* succeeded one another in time with overlapping temporal distributions (e.g., $A > A(B) > B > B(C) > C$), it remains forbidden that—for example—*C* should appear before *A* at any exposure (although it can be expected that *B* will sometimes appear before *A*, and *C* before *B*—just not that often).

To what extent can cross-checks of this sort vindicate the claim that assemblages that succeed one another in strata also succeeded one another in time? Certainly they cannot *prove* this. Even if every observed succession is compatible with the hypothesis that *A*, *B* and *C* succeeded one another in time, this does not establish that they in fact did so. Perhaps in every case the apparent temporal succession was due to accidents of preservation, and *A*, *B* and *C* in fact existed for exactly the same interval. Or perhaps *A*, *B* and *C* *did* succeed one another in time, but only at the examined sections. In other, unexamined sections, *B* existed well before *A* and endured long after *C*. There is nothing conceptually incoherent about these proposals, but the crucial point is that they become less plausible as more stratigraphic sections are examined. Once Thomas Jefferson hoped that Mastodons might survive in the vast American interior, but as more of the country was explored, this hope became difficult to sustain. In a like fashion, some geologists in

the 1830s were happy to postulate that land plants might have existed in the Cambrian Period; but by the 1850s, these notions had been mostly confined to the fringes of the geological community (Rudwick 1985).

The reason they had become untenable was the absence of certain kinds of evidence—in particular, evidence of land plants interbedded with Cambrian marine fossils. Consider that to postulate that *A* and *B* coexisted for a significant period of time is to suggest the likelihood that at some exposures, at least, members of *A* should be found in association with members of *B*—in particular, if either *A* or *B* contains a taxon that is (1) widespread in distribution and (2) abundantly preserved in a variety of depositional environments (Harper 1980). If members of *A* and *B* are *not* observed in association at any exposure, the claim that *A* and *B* coexisted for a significant period of time becomes harder to swallow, and may come to seem indefensible as more exposures are examined. The claim can never be disproved using fossil evidence alone (perhaps land plants *did* exist in the Cambrian, despite never being observed in conjunction with any characteristic Cambrian animals). Yet at some point, the failure to observe *A* and *B* in association will tip the balance of evidence in favor of the claim that *A* and *B* did not coexist for a significant period of time. Notice that when *A* and *B* are taken to be successive assemblages, this pattern of reasoning can lend support to the claim that *B* succeeded *A* at approximately the same time throughout its range: that for the purposes of stratigraphic correlation, the transition from *A* to *B* can be taken to mark a time-horizon wherever it is preserved.⁸

⁸ Again, the reason is that, were the transition from *A* to *B* not roughly synchronous throughout their respective ranges, we would expect to find members of *A* and *B* preserved in association at some exposure(s).

So, one can use features of documented homotaxial patterns (in particular, their invariance over a sampling area) to infer the probable existence of real temporal successions. Moreover, by factoring in additional features (like the geographic range of marker taxa), one can make certain inferences about absolute synchronicity, at least within a reasonable margin of error, corresponding to a far shorter amount of time than the lifespan of an average taxon. Negative evidence can bolster these inferences (with usual caveats), since highly staggered successions are expected to produce telltale associations at certain exposures. Taken together, this is encouraging.

Still, it would be nice if there was some independent way to evaluate particular correlations. For example, if we could measure the age of at least some strata without using fossils, then we could check and see whether fossils were indeed a reliable guide to long-distance correlation. Today this is possible through a variety of absolute dating techniques and alternative methods of correlation. But in the nineteenth century, unfortunately, absolute dating methods were unavailable. To assign even a relative date to a rock formation, fossils were almost everywhere the best bet.

Yet there were multiple ways of using fossils to measure time, at least for the youngest strata (the so-called “Tertiary formations”). Charles Lyell, for instance, proposed to measure the relative age of rocks using “the common relation which [their fossil contents] bear to the existing state of the animate creation” (Lyell 1835, 58). The thought here is simple. If species tend to go extinct at something like a constant rate, then one can use the *ratio* of extinct to extant species in a formation to assign that formation a relative age. Formations containing a larger percentage of extant species are likely to be younger than those containing a smaller percentage of extant species, all else being equal.

Mollusks supplied the chronometer of choice because of their numerical abundance. (In a less abundant group, biases of preservation could interfere with the temporal signal from extinction.) So Lyell set to work tallying the mollusks of the Tertiary formations—or rather, he contracted a paleontologist to do it—resulting in his famous division of the Tertiary Period into the Pliocene, Miocene and Eocene Epochs (Rudwick 2008).

How does this provide an independent check on paleontological correlations? Both strategies, it is true, use fossils to measure time. But they use fossils in different ways; and this provides an opportunity to cross-check their results. For Lyell, what matters is the ratio of extinct to extant species, not the particular taxonomic identities of species in a stratum.⁹ For Smithian stratigraphy, by contrast, specific taxonomic identity is everything.¹⁰ This suggests that if two strata correlated on the basis of characteristic fossils *also* contain a similar ratio of extinct to extant species, then we have two independent lines of support for the correlation. But did Lyell or anyone else actually perform cross-checks of this kind?

Yes—sort of. Lyell was a friend of Smithian stratigraphy, and for this reason probably felt no great need to explicitly verify Smithian methods (Rudwick 1985). Still, several comments in *Principles of Geology* suggest that he was aware of the value of such cross-checks (or at least that he perceived that an agreement with Smithian correlations would add credibility to his chronometer). In discussing “[the] Eocene strata of Paris and London,” for example, Lyell notes that they “are marked by the presence of a

⁹ Moreover, as Charles Darwin observed, “[The success of this method presupposes] that the rate of change is everywhere the same...[and] that species become extinct in [the] same ratios over the whole world” (Allmon 2016, 684).

¹⁰ Here the key assumption is that fossil assemblages succeed one another at about the same time wherever in the world they are found.

vast variety of peculiar extinct species of testacea [shelled mollusks], as well as of other animal and vegetable remains” (Lyell 1835, 397). This indicates that Smith’s method of characteristic fossils can be used to correlate these formations. At the same time, “it should be observed that had the geologist collected the fossils of the crag in Norfolk, the blue clay of London, and the white limestone of Paris, and [considered] these formations merely with reference to the number of recent [extant] shells contained in each, he would have seen...that the Parisian and London strata differed widely from the crag, and agreed very closely with each other.” In other words, measurements of the ratio of extinct to extant mollusks in the Paris and London strata agree with the correlation of these strata on the basis of characteristic fossils. So two methods, each employing different central assumptions, returned the same measurement. This is an example of a successful “coherence test” in the sense outlined by Bokulich (2019).¹¹

Did geologists solve their problem of nomic measurement, then? In a sense they did. To solve the problem, geologists needed to show, first, that the fossil record preserves a directional signal, and second, that events in the record taken to mark time-horizons were roughly synchronous over the relevant areas. And by the middle of the nineteenth century, both of these claims had been rendered increasingly plausible. In both cases, the reasons for supporting the claim flowed not from an overarching theory, but instead from judgments of plausibility anchored in knowledge of local stratigraphic sections. Yet they were none the weaker for this—and in fact, the absence of a widely recognized theory of paleontological correlation probably saved the practice from

¹¹ In a coherence test, two independent methods are used to produce a measurement (e.g., a date). The methods are then assessed for convergence, not in order to generate a more accurate measurement, but rather to probe for possible sources of error.

disruption, since the most celebrated theories of early nineteenth century geology were neither universally accepted nor particularly long-lived.

6. Conclusion

This chapter has been about a start-up problem in scientific practice: how were geologists in the nineteenth century able to solve their problem of nomic measurement? Roughly speaking they had two options. The first was to articulate a theory that showed that the fossil record preserves a directional signal and that faunal transitions preserved in the record were roughly synchronous over large geographical areas. The other was to warrant these claims in the absence of an overarching theory. Contrary to the expectation that epistemically successful methods require the existence of approximately true scientific theories, geologists took the second route in this case and were successful in doing so. Their success did not place their practice beyond the reach of all doubt, as Huxley's criticisms suffice to show. Yet by the middle of the nineteenth century, most reasonable doubts about the practice had effectively been assuaged.

What, if anything, does this teach us about how scientists operate in start-up situations? I think there are two philosophical lessons here, one relatively obvious, the other less so. The obvious lesson is that methods can be judged to be epistemically reliable in the absence of a justifying theory—or at least reliable enough to warrant continued application in a domain. In start-up situations, what matters is not whether a method meets a stringent criterion of epistemic reliability; rather, it is enough that it be shown to be *preferable to other methods* in terms of its ability to handle outstanding problems and generate fruitful research. Justification in start-up situations, in other

words, is comparative and pragmatic. Possibly this is a feature of scientific justification more generally. Most scientific research is unfinished, and most of this occurs at the frontiers of advancing knowledge, where the *terra firma* has long ago yielded to the *terra incognita* (Nickles 2009; Guttinger and Love in preparation). In this soggy and bracing environment, scientists cannot afford to be overly exacting in their standards of justification. That is, in the “context of pursuit,” much justification depends on methods of heuristic appraisal as opposed to methods of retrospective assessment (see Laudan 1980; Wimsatt 1981; Nickles 1988).

The less-obvious lesson is an elaboration of this final point. In start-up situations, we can expect matters of justification to be keyed to forward-looking judgements of heuristic utility as opposed to retrospective assessments of epistemic success. Nickles (1987) calls the quality that is scrutinized in processes of heuristic appraisal “generative potential.” It consists in the ability of a methodology or research program “to handle problems still outstanding and to generate interesting new questions for research” (Nickles 1987, 47). In early nineteenth century geology, judgements of generative potential clearly favored the fossil-based research program (Rudwick 1985; Herbert 2005). In its ability to handle apparent anomalies and to generate new questions for research, it had no serious competitors after the 1830s. Doubtless this is a key to explaining its success, even before the piecemeal justification of particular correlations could shore up its flanks. Paleontological correlation may have had uncertain epistemic *bona fides*, but after some early anomalies were dispatched, it was—and remained—geologists’ best bet.

Chapter 3:

Explaining the Apocalypse: The End-Permian Mass Extinction and the Dynamics of Explanation in Geohistory

1. Introduction

Explanation is a perennially hot topic in philosophy of science (Salmon 1989; Woody 2015). Since the middle of the last century, philosophers have shown a keen interest in what scientific explanations are, what distinguishes good from bad explanations, and what makes explanatory power a theoretical virtue (if it is one). In addition, many philosophers have become interested in the question of how scientific representations contribute to explanation despite containing prominent idealizations and abstractions (e.g., Potochnik 2017). In scrutinizing these issues, philosophers have tended to regard explanations in one of two ways: either as formal arguments (as in the deductive-nomological framework) or as communicative acts (e.g., answers to why-

questions or representations of causal patterns).¹ Yet the topic of explanation presents many aspects, not all of which yield to a focus on individual explanations (Woody 2015). In particular, this focus fails to illuminate the temporal dimension of explanation *considered as an open-ended and collaborative process*. Here we are not concerned with single explanations, but rather with the temporally extended activity of explaining complex phenomena or systems. Here too we are not concerned with single explainers (or at least we needn't be), but rather with teams of explainers working jointly or in competition. In short, we are concerned with the *dynamics of explanation*—a subject that has received surprisingly little attention, despite its evident importance for understanding how science works.²

This chapter is about the dynamics of explanation in geohistory. More specifically, it is about how geohistorical explanations develop over time, as well as certain of the factors responsible for generating this pattern. The chapter takes as its jumping-off point an observation from Currie (2014): that explanations of “[h]ighly contingent, disunified events” tend to “shift from simple to complex as time goes by”

¹ A third option is to regard explanations *ontically*: that is, as nonrepresentational physical entities as opposed to representations of those entities (Wright and van Eck 2018). The present discussion presumes a non-ontic—that is, a representational—view of explanation.

² I do not mean to imply that the dynamics of explanation has received no philosophical attention. The vast literature on scientific reduction contains insights on how theoretical explanations change over time (see van Riel and Van Gulick 2019). In addition, philosophers interested in mechanisms have gone to great lengths to explore the process by which mechanistic explanations are constructed and refined (e.g., Bechtel and Richardson 1993; Craver and Darden 2013). The latter is the kind of inquiry I have in mind when I speak of the dynamics of explanation.

(1173).³ Yet it goes beyond this observation by seeking to account for this pattern in terms of investigative processes involved in characterizing complex phenomena, as well as their interaction with allied explanatory practices. I argue that to understand the dynamics of historical explanation, it is necessary to attend to various kinds of *non-explanatory work*—work that is undertaken to increase our descriptive understanding of a phenomenon, not to test a particular explanatory claim (Feest 2017). Doing so reveals that an important reason historical explanations “shift from simple to complex” over time is that non-explanatory work tends to multiply the demands placed on would-be explainers. As a result, the complexity of explanatory hypotheses tends to increase—although as I suggest in this chapter, the pattern is sometimes violated for reasons that are both explicable and important for understanding the dynamics of explanation in general.

My discussion of the dynamics of explanation makes contact with another important topic in philosophy of science: the problem of what organizes research into complex phenomena. According to an old philosophical tradition, the main organizational scaffolding of scientific research is provided by explicitly articulated theories and models (Fagan 2011; Love 2014). These structure research by setting a context of expectation that tells researchers what to attend to and what to ignore. Yet in the historical sciences, explicitly articulated theories and models are hard to come by (Currie 2018). Historical scientists thus need something else to “guide the search for relevant new facts” (Currie and Sterelny 2017, 17). Happily, hypotheses or “narratives” can play this role, “[extending] our reach into the past...by enabling the identification of relevant evidence”

³ The notion of complexity in focus for Currie is causal complexity: an explanation is complex in proportion as the causal structure it represents is complex. When I speak of the complexity of explanations in this chapter, it is this notion of complexity I intend.

(ibid., 19).⁴ Given the prevalence of underdetermination in the historical sciences (which makes the problem of identifying relevant evidence especially acute), it follows that a majority of research in the historical sciences should be oriented around the testing of explanatory hypotheses (see, e.g., Turner and Currie 2017).

As I indicated, this chapter is primarily about the dynamics of explanation in geohistory. Yet the account I develop does not cohere with the suggestion that most historical research is oriented around the testing of explanatory hypotheses. I do not deny that historical scientists propose and evaluate explanatory hypotheses—but this does not mean that most historical scientists, most of the time, are laboring to produce evidence that either confirms or disconfirms particular explanatory claims.⁵ Instead, they are engaged in a variety of activities that may be relevant to the evaluation of explanatory hypotheses, but that have different immediate goals, like the construction of a high-resolution time scale for understanding an event (Erwin 2006b). To achieve a more rounded perspective, I propose that historical reconstruction can be analyzed in terms of *structured problem agendas*—suites of research tasks and associated criteria that pertain to different aspects of complex phenomena (Love 2008, 2014; Currie 2019a). Seen in this light, historical reconstruction is a distributed activity, and non-explanatory work is a

⁴ “Hypothesis” refers throughout to a claim assessed as (probably) true or false in light of evidence (Fagan 2011). A narrative is a kind of hypothesis that purports to explain an event by adducing a sequence of events that leads up to, or comprises, the focal event (Currie 2014).

⁵ Some are. Erwin and Vogel, for example, tested the causal relationship between volcanic eruptions and extinctions by examining the temporal proximity of “the largest, best constrained pyroclastic events” and extinction levels, and found no correlation, ostensibly disproving an explanatory hypothesis (Erwin and Vogel 2014, 893).

vital mode of participation in a project whose internal structure serves to distribute the epistemic burden over a large (and typically multidisciplinary) community of researchers.

The remainder of this chapter is organized as follows. In Section 2, I explore Currie's claim "that historical science exhibits a progressive pattern [from simple to complex explanations]," as well as his claim that, in the early stages of an investigative project, a majority of hypotheses will be simple "one-shot hypotheses" (Currie 2019b, 8). I suggest that while Currie provides a plausible *kinematics* of historical explanation—an analysis of pattern—his account of the relevant dynamics is less satisfactory. To begin the task of articulating a more adequate dynamics, I explore one of the major investigative projects in the contemporary geosciences: research into the end-Permian mass extinction. Section 3 explores three phases in the explanation of the extinction, encompassing an increase, and then a decrease, in the complexity of explanatory models. Section 4 explores what factors were responsible for this trajectory, paying special attention to the role of non-explanatory activities. In Section 5, I consider what the case of the end-Permian mass extinction teaches us about the dynamics of explanation in geohistory; I also consider how the concept of a structured problem agenda can help us in thinking about the organization of geohistorical research and the dynamics of explanation more generally. Finally, in Section 6, I provide a brief synopsis of the main arguments of the chapter.

2. Historical Explanation: From Kinematics to Dynamics

According to Currie, explanations of historical events and processes "shift from simple to complex as time goes by" (Currie 2014, 1173). This pattern of progressive

development has three stages. First, “a series of simple narratives are generated”; then, “[the narratives] are explored and tested against the historical record”; and finally, “[the] surviving narratives are woven together into complex narratives” (Currie 2019b, 8). Or as Currie writes in his (2014):

- (1) A series of competing simple narratives are put forward
- (2) Some hypotheses are abandoned due to empirical failings
- (3) Surviving hypotheses are synthesized into complex narratives (1175)

Currie (2014) is careful to note that this pattern is not without exceptions: “presumably there are cases where simple narratives are entirely appropriate,” and there is no reason for researchers to move from step (2) to step (3). In addition, Currie does not say that progress in the historical sciences is limited to the synthesis of more complex narratives out of simpler ones. Along with articulating narratives of increasing complexity, a “complete” picture of progress in the historical sciences would include things like “the incorporation of new technologies and methods for uncovering traces and testing hypotheses, the postulation of new explanations, the incorporation of theories from other sciences and the explanation of historical regularities” (Currie 2014, 1176). Yet when it comes to the pattern of historical explanation, Currie thinks that his three-stage pattern will exhibit a dominant relative frequency, at least when the explanatory target is a “complex, contingent episode” (Currie 2019b, 1). Nearly all attempts to explain complex, contingent episodes will begin with a giant burp of explanatory hypotheses, followed by a process of sifting and rejection, and culminating in a period of synthesis, wherein surviving hypotheses are reconciled and set in a more complex narrative structure. Exceptions to this pattern are possible, but are unlikely to be very common.

I regard Currie's account as providing a kinematics of historical explanation; it is an account of the trajectory of historical explanations that is little concerned with the "forces" underlying that trajectory. Currie writes, for example, that "historical explanations shift from simple to complex as time goes by" (Currie 2014, 1173). This is a statement about pattern. It concerns the development of explanatory projects over time, but it does not say *why* historical explanations exhibit a trend towards greater complexity—it simply observes that they do. A dynamics of historical explanation, by contrast, answers the why question. It explains why historical explanations tend to grow more complex in terms of the factors responsible for generating this pattern. Certain of Currie's statements suggest that he is interested in this dynamical question. In his (2014), for example, Currie observes that Carol Cleland's account of historical reconstruction "does not *explain* the shift from simple to complex narratives" (1176, emphasis added). Currie's own account, by contrast, does explain this feature, since it emphasizes not only "the empirical rejection and confirmation of hypotheses," but also their amalgamation to form new, more complex hypotheses (i.e., narratives). Here then is the outline of a dynamics: one that is centrally concerned with the testing, rejection and amalgamation of explanatory hypotheses.

I am prepared to accept Currie's account of the kinematics of historical explanation; historical explanations really *do* become more complex as time goes by, at least in a large number of cases. Yet I am less satisfied with his observations regarding its dynamics. The reason is the central place they award to the formulation and testing of explanatory hypotheses ("narratives"), and their consequent neglect of non-explanatory work—work that may be relevant to the evaluation of explanatory hypotheses, but that is

not undertaken in the interest of testing any particular explanatory claim.⁶ Non-explanatory work is a bit of a wastebasket category, and includes a variety of projects in stratigraphy, geochronology, geochemistry, taphonomy and systematics, to name just a few areas.⁷ Yet it is worth naming since these projects constitute a major focus of research in the geohistorical sciences, and since their primary goal is to *characterize* complex phenomena, as opposed to producing evidence bearing on an explanatory claim. An important thesis of this chapter is that non-explanatory work is every bit as important in determining the trajectory of explanatory projects as hypothesis-testing itself, and indeed more important in some cases. Further, it has “a life of its own,” to steal a phrase from Ian Hacking (1983). Non-explanatory work is not just a sideshow to the “real business” of hypothesis-testing.

Currie is not deaf to the significance of non-explanatory work. In his discussion of “one-shot hypotheses” (see below), Currie highlights the importance of studies that aim to explore the dynamics of hypothesized causal factors (Currie 2019b, 15; see also Novick et al. 2020). These studies fit my definition of non-explanatory work since they are designed to increase our descriptive understanding of a phenomenon (as opposed to

⁶ Currie (personal communication) suggests that he is giving an ontic account of the focal pattern: since history is causally complex it demands a complex explanation, which scientists come to realize when their attempts to confirm simple hypotheses end in failure. This may be; still, his account contains observations about the investigative practices involved in the explanatory process, and it is here that I find it wanting.

⁷ These projects count as “non-explanatory” for the simple reason that they have as their immediate aim the exploration or description of a subject domain, *not* the testing of an explanatory hypothesis (see Feest 2017). In some cases, it may be hard to say whether a project’s aim is description, explanation or both. That is fine: all I wish to achieve with my category of “non-explanatory work” is to pick out a set of practices whose proximate aims are straightforwardly descriptive.

testing an explanatory claim). My quibble is not that Currie ignores non-explanatory work; it is rather that he gives it insufficient weight in articulating an idealized view of historical explanation. My goal is to explore a less idealized view: one that gives due weight to the interaction of characterization and explanation that drives so much research in the sciences of geohistory.

There is another element of Currie's account that warrants mentioning. Early in the career of an explanatory project, a majority of hypotheses will be "one-shot hypotheses," Currie thinks (Currie 2019b). These are monocausal explanations "of complex, contingent [events]" that researchers "treat as mutually exclusive with other possible explanations of the [event]" (1). They are also simple. Each one-shot hypothesis "takes a complex history and accounts for it with a single factor" (2). This makes the prevalence of one-shot hypotheses puzzling, since the past is complicated—and if the past is complicated, then why should historical scientists prefer hypotheses whose very simplicity makes them unlikely to be correct? Currie's response is that these simple hypotheses provide two kinds of productive scaffolding. First, they help scientists isolate hypothetical difference-makers: "empirically tractable dependencies between variables," which may or may not be relevant to explaining a focal event (1). Second, they provide "raw materials" for constructing "more complex—and likely more adequate—explanations." Scientists are not justified in their preference for one-shot explanations because these hypotheses are likely to be correct; rather, they are justified in their preference because one-shot explanations facilitate fruitful inquiry and learning, regardless of whether they are correct or not.

Putting all this together, we can distinguish three main questions that arise out of

Currie's account of historical explanation. These are:

- (1) What accounts for the fact—if it is a fact—that early in the career of an investigative project, many hypotheses are simple, “one-shot hypotheses”?
- (2) What accounts for the fact—if it is a fact—that historical explanations shift from simple to complex as time goes by?
- (3) Is the practice of articulating one-shot hypotheses reasonable given that the world is complex, and therefore unlikely to yield to simple hypotheses?

In this chapter I will concentrate on questions (1) and (2). Given my answers to these questions, question (3) does not arise, or at least does not arise with the same urgency as it does for Currie. Nonetheless, I will touch on the justification of one-shot hypotheses in Section 5, when I take a second look at Currie's characterization of “one-shot hypotheses.”

So far, this discussion of historical explanation has been highly abstract. An example can help us see these issues in the flesh, and will guide us in answering the several questions outlined above. With this in mind, let us turn to scientists' attempt to explain the most devastating event in the history of life on Earth: the end-Permian mass extinction, some 252 million years ago (Ma).

3. Explaining the Apocalypse

The end-Permian mass extinction was the greatest biological calamity in the history of the planet. It is estimated that around fifty percent of marine families and

perhaps ninety percent of marine species perished in the debacle—a loss of diversity unequaled in any other extinction event (Raup 1979; Jin et al. 2000). On land, more than sixty percent of vertebrate families seem to have vanished, along with an unprecedented number of insects (Labandeira and Sepkoski 1993; Benton 1995).⁸ So profound was the destruction that the biosphere lay mostly fallow for several million years after the extinction, and after that wore a strikingly different aspect. In the words of Douglas Erwin, one of the foremost experts on the extinction: “the Permo-Triassic [P–Tr] boundary [was] a fundamental turning point in the history of life, bringing the world of the Paleozoic to a close, and, in the aftermath of the extinction, constructing the world of today. Despite all the evolution of the past 251 million years, today’s oceans still reflect the winners and losers of events at the close of the Permian” (Erwin 2006b, 7).

Stephen Jay Gould once remarked that no problem in paleontology has attracted more attention than the causes of mass extinctions. “The catalog of proposals would fill a Manhattan telephone book and include almost all imaginable causes: mountain-building of worldwide extent, shifts in sea-level, subtraction of salt from the oceans, supernovae, vast influxes of cosmic radiation, pandemics, restriction of habitat, abrupt changes in climate, [et cetera]” (Gould 1977, 134). Almost all of these causes were offered as explanations of the great Permian extinction—especially prior to the 1970s, when geologists’ overall picture of the event remained poorly drawn (see Rhodes 1967 for an early review of the literature). Beginning in the 1970s, however, a tentative consensus on

⁸ The evidence for a mass extinction among plants is more equivocal, with a recent study finding no evidence of a Late Permian mass extinction (Nowak et al. 2019; for a differing view, see Retallack 1995). Still, the Permo-Triassic boundary marks a major change in floral patterns, and no coal seems to have formed in the first 10 million years of the Triassic, suggesting a major impoverishment of the flora (Benton and Newell 2014).

the cause(s) of the extinction began to solidify. Peter Ward describes this consensus as follows:

By the end of the Paleozoic Era, some 250 million years ago, there was but a single “supercontinent” [Pangea], composed of a united North America, Europe, Asia, and Africa. Two effects of this gigantic, tectonic embrace...produced the extinction. First: The Earth’s climate had changed. Because of its immense size, huge areas of the supercontinent could no longer be cooled or warmed by steady maritime influence, and the interiors of this gigantic continent thus grew hotter in summer and colder in winter. Second, when the contents coalesced, the level of the oceans fell dramatically, causing the wide interior seas found on virtually every continent at that time to disappear. It was within these shallow seas that most Paleozoic marine life had lived. (Ward 2004, 7)

The two processes, Ward observes, “were linked.” As the climate of Pangea grew more arid, shallow seas evaporated more quickly, and this led to further aridification since inland seas have an ameliorating influence on climate. At the same time, falling sea-levels (produced in part by the merger of the continents, which put a brake on seafloor spreading) drained the shallow seas surrounding Pangea, further reducing the living space of shallow-water organisms (Valentine and Moores 1970, 1973; Schopf 1974). The destruction was indiscriminate. Among the denizens of shallow water marine environments, “the extinctions appear to [have] affect[ed] many marine groups in a rather similar manner” (Schopf 1974, 139). It was also slow. Apropos of an event caused by the gradual deterioration of climate and the associated draining of shallow-water habitats, Permian extinctions were scattered over a period of perhaps ten or twenty million years with a crescendo at the end. No catastrophe this—the end-Permian extinction was a protracted affair, in contrast to the spasm of death that brought the Cretaceous Period to a close.

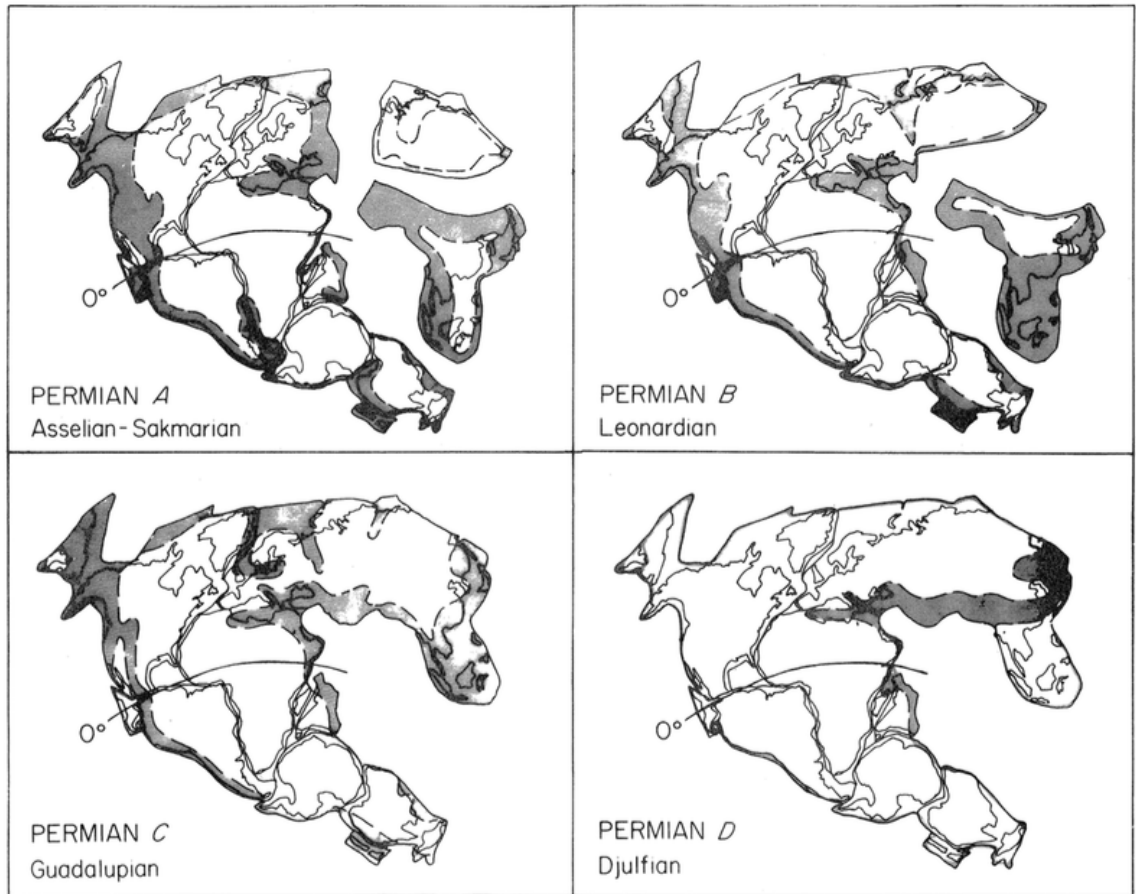


Figure 4 A reconstruction of continental positions during the Permian, showing a major reduction in the area of shallow marine seas (shaded regions) from the earliest (“Permian A”) to the latest (“Permian D”) Permian. (From Schopf 1974)

There are three things to notice about this explanation:

1. The overall model is relatively simple. Continents once separated by oceans fuse into a single landmass, slowing seafloor spreading and causing a marine recession that drains the inland seas and the continental shelf. At the same time, the fusing of continents into a single landmass alters the climate, increasing seasonality and further accelerating the disappearance of the shallow seas. There is more than one process at work here (e.g., plate tectonic movement, marine recession, climate

change), yet the extinction model unites them into a relatively straightforward mechanism, with a reduction in living space and biotic provinces as its focal point.

2. The kill mechanism is primarily designed to knock off shallow-water marine organisms. This is because, during the 1970s and 1980s, the end-Permian extinction was thought to have “primarily affected marine organisms”; “[the] relatively few terrestrial plants and vertebrates,” by contrast, “[were] not so strongly disturbed” (Gould 1977, 137). Two analyses of the fossil record of terrestrial vertebrates, the first by C.W. Pitrat (1973) and the second by Everett C. Olson (1982), found no evidence of a mass extinction around the Permian-Triassic boundary. As such, there was no reason to fret about whether a proposed kill mechanism could produce an extinction of land animals and plants (although increased extremes of temperature and altered nutrient availability could be invoked to do away with those lineages that were known to have gone extinct near the P–Tr boundary).
3. The model is designed to explain an extremely slow-moving crisis, with individual extinctions staggered over a period of ten million years or more. As the paleontologist and expert on P–Tr boundary sections Curt Teichert observed in 1990, “The way in which many Paleozoic life forms disappeared towards the end of the Permian Period brings to mind Joseph Haydn’s Farewell Symphony where, during the last movement, one musician after the other takes his instrument and leaves the stage until, at the end, none is left” (231). Because of this, no catastrophic agent is needed to produce the extinction; instead, what is needed is a mechanism capable of producing a steady trickle of extinctions over a prolonged period.

Fast forward to the early 1990s, and the “tentative consensus” described above had begun to fray. Investigators continued to regard the extinction as a lengthy one—Holser and Magaritz (1992), for example, put the length of the extinction between 5 and 10 million years (making it the longest of the Phanerozoic mass extinctions). And nearly everyone remained convinced that it was accompanied by a drop in sea-level, perhaps exceeding 250 meters (Holser and Magaritz 1987). Yet new lines of evidence “from virtually every branch of the earth sciences” had begun to significantly complicate the overall picture, imposing a range of new demands on would-be explainers (Erwin 1996, 74).⁹ In response, explanatory models began again to proliferate. Perhaps the extinction was caused by volcanic outgassing associated with the eruption of the Siberian flood basalts—the largest continental volcanic episode of the past half-billion years (Campbell et al. 1992).¹⁰ Or perhaps it was caused by increased levels of marine anoxia brought on by the erosion of the exposed continental shelf (Holser et al. 1991). In any event, it was clear that an updated model of the extinction was sorely needed, and that any model worth its salt would have to be consistent with the new evidence pouring in from across the geosciences, including the emerging isotopic record of the Late Permian (see Section 4.1).

Both the excitement and the confusion of this period are encapsulated in Erwin’s

⁹ For example, new continental reconstructions indicated that the supercontinent Pangea formed during the *Middle* Permian, too early to have caused the extinction by drift-induced reduction in marine shelf area (Erwin 2006). Still, the idea that the extinction owed to a reduction in marine living space held on (with falling sea-levels implicated in the real estate crash), and likewise the idea that Pangea had something to do with the extinction, especially on land.

¹⁰ This could have triggered an extinction by bringing about rapid global cooling, and perhaps influencing sea-levels (see Campbell et al. 1992).

1993 monograph, *The Great Paleozoic Crisis*. In this work, Erwin reviews the evidence pertaining to the end-Permian extinction (paying special attention to new evidence, like the evidence from isotopic studies), and uses it to construct a novel “scenario for the end-Permian mass extinction” (Erwin 1993, 255). He calls this scenario the “Murder on the Orient Express hypothesis,” after the Agatha Christie story in which the eponymous murder is carried out by all the passengers on the train working together.¹¹ In a like vein, Erwin sees the end-Permian extinction as the work of “a multitude of events occurring together, in particular the increased climatic and ecological instability associated with the regression [sea-level drop] and a combination of greenhouse warming and possible oceanic anoxia from increased atmospheric CO₂” (Erwin 1993, 256). Or as he describes the model in his (1994):

The extinction began with the loss of habitat area as the regression dried out many marine basins, converting the two-dimensional coastlines of the mid-Permian into more linear coastlines. The increased exposure of Pangea as the regression progressed exacerbated climatic instability. This instability, coupled with the effects of continuing volcanic eruptions and an increase in atmospheric carbon dioxide (with some degree of global warming), led to increasing environmental degradation and ecological collapse...Some degree of oceanic anoxia may [also] have developed...The final phase of the extinction occurred in the earliest Triassic. The rapid transgression [sea-level rise] destroyed near-shore terrestrial habitats, causing the shifts in spores and pollen and perhaps much of the decline in insects and tetrapods. (Erwin 1994, 235)

It is clear that this explanation is more complicated than the earlier one. In addition to regression and increased seasonality, we now have volcanoes pumping carbon dioxide into the atmosphere, (probable) marine anoxia and (not mentioned in the above

¹¹ *Spoiler alert.

quotations) the release of methane gas hydrates from a deep ocean reservoir. These new processes are not “overkill,” so to speak—they arose from particular demands of the evidential situation, and were needed to explain all the pertinent evidence. Yet in spite of this, many paleontologists found Erwin’s model difficult to swallow. Maybe the end-Permian extinction was produced by “fateful combination of...several misfortunes,” with regression and anoxia knocking off the marine invertebrates, and extremes of temperature, followed by a later (Triassic) transgression doing in the terrestrial vertebrates and insects (Fortey 1997, 2007). But other investigators preferred to lay the blame at the feet of an extraterrestrial object, or to ascribe a larger share of the responsibility to the flood basalt eruptions in what is now Siberia. In any event, all parties agreed that more research was needed: for example, better biostratigraphic correlations, more isotopic studies with improved stratigraphic sampling, and new ways of estimating the amount of CO₂ and SO₂ released by flood basalts (see Erwin 1993).

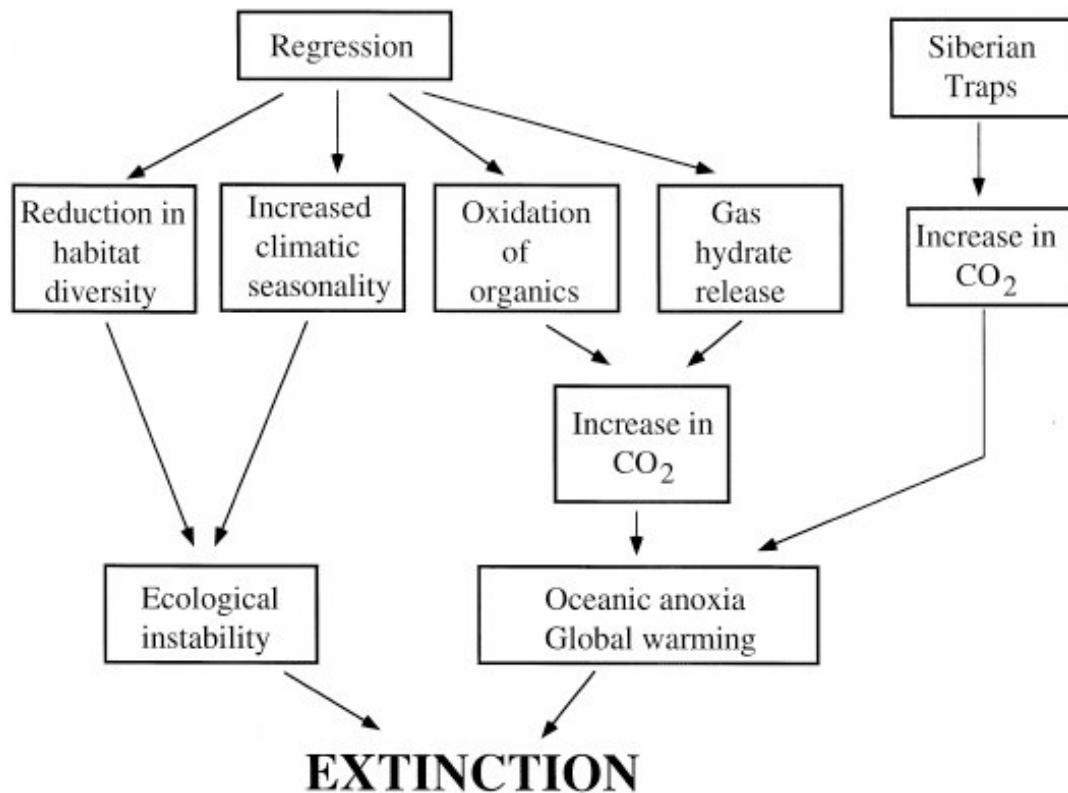


Figure 5 The *Murder on the Orient Express* hypothesis, with its dual triggers (marine regression and Siberian Trap volcanism) and multiple downstream kill mechanisms. Notice that the Siberian Trap eruptions are here represented as very much a secondary trigger; a far more consequential trigger is the marine regression. (From Erwin 1993)

Fast forward again, this time to the present day. The end-Permian mass extinction is universally recognized to be a complex affair involving the interaction of multiple earth systems and the biota. But it is now known to have been incredibly rapid as well, with land and sea extinctions taking place at roughly the same time. Partly for these reasons, the *Murder on the Orient Express* hypothesis (with its three phases spread over millions of years) has been abandoned. In its place, “a consensus has emerged that Siberian trap volcanism caused the extinction via $p\text{CO}_2$ changes and possibly CH_4

[methane] influx, leading to hypoxia [insufficient oxygen], global warming, and the expansion of anoxia in OMZs [oxygen minimal zones]” (Wood and Erwin 2018, 870; see also Wignall 2015; Benton 2018).¹² The “consensus” does not command complete assent, especially when it comes to the involvement of methane gas hydrates. Yet while significant uncertainties remain, it is noteworthy that the new picture represents a streamlining of the scenario presented in Erwin (1993), “in which unrelated bad things happen together by chance” (Knoll et al. 2007, 308).

¹² More precisely, the eruption of the Siberian Trap basalts is believed to have liberated vast amounts of carbon dioxide and sulfate aerosols, which led to runaway global warming and increased acid rainfall. It may also have liberated large amounts of methane (a potent greenhouse gas), either from contact metamorphism or from the destabilization of methane gas hydrates in underwater reservoirs. The combined effect of global warming and acid rain was to denude the landscape, causing massive erosion that flushed the shallow seas with nutrient rich soils and siliciclastic debris. This led (by an uncertain mechanism) to the spread of anoxic and sulfidic conditions in the sea, precipitating the marine extinction, while terrestrial extinctions “presumably resulted from aridity, acid rain, loss of soils, and perhaps short-term effects of wildfires and damage to the ozone layer” (Benton and Newell 2014, 1314).

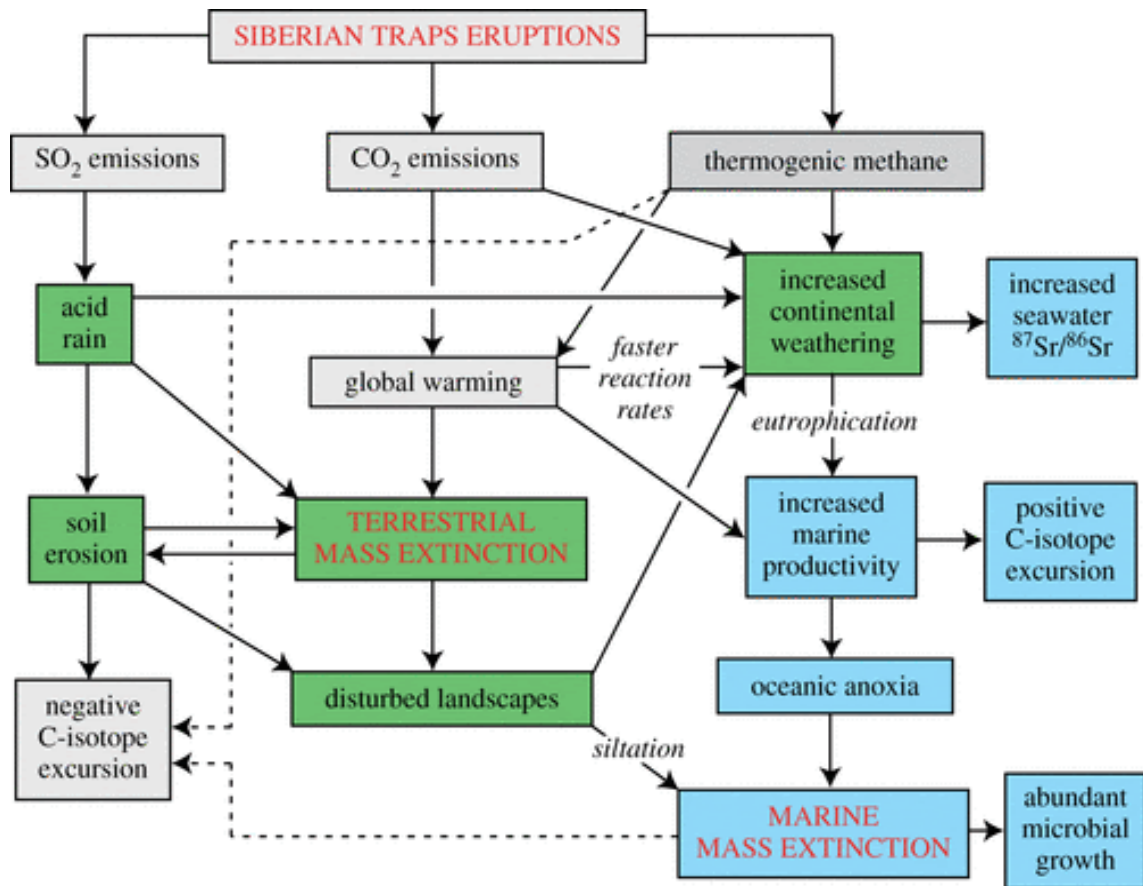


Figure 6 A new “consensus” model for the end-Permian mass extinction. In this figure, causal links are depicted with solid arrows, and possible second-order controls with dashed lines. Notice that, while the figure contains more boxes than Figure 5, the extinction mechanism itself has been streamlined, with all effects flowing from a single triggering cause. (From Benton and Newell 2014)

So have explanations of the end-Permian mass extinction grown more complex over time? This question is difficult to answer, since there have always been multiple hypotheses on offer (not equally well-supported, but still “live options”), and we lack an objective measure of the complexity of a hypothesis. Still, I would venture to make two preliminary claims.

First, the recent “consensus” on the explanation of the end-Permian extinction is not significantly more complicated than the original one. Each posits a single trigger (volcanic outgassing and the formation of Pangea, respectively) that sets in train a variety of downstream kill mechanisms. The number and variety of enlisted kill mechanisms is probably greater in the newer model (different articulations of each model differ in the particular processes they incorporate), yet the overall difference in complexity is a fairly modest one.

Second, the *Murder on the Orient Express* hypothesis is more complicated than either of the two “consensus” hypotheses. Assuming that the *Murder on the Orient Express* hypothesis was the best-supported extinction model in the early nineties (a not unreasonable assumption), the complexity of best-supported hypotheses thus describes a convex upward curve. The question then becomes *what accounts for the initial complexification and later streamlining of explanatory hypotheses for the end-Permian mass extinction?* The answer to this question illustrates a general point about the dynamics of explanation for complex historical phenomena.

4. Characterizing the Apocalypse: Non-Explanatory Work as a “Driver” of Explanation

Currie (2014, 2019b) claims that historical explanations tend to become more complex as time goes by. He further claims that this pattern owes to the “exploration” of an initial pool of hypotheses, followed by the amalgamation of non-refuted alternatives into more complicated narratives. I accept this pattern, but am interested in developing a richer account of the dynamics of explanation in geohistory: one that gives appropriate

weight to all the activities that contribute to the process of historical explanation, and that can account for both the complexification and the streamlining of causal models. This section begins this work, using research into the end-Permian extinction to illustrate the basics of the new picture.

The main claim I wish to illustrate is that the principal “driver” of explanations for the end-Permian extinction is non-explanatory work: things like stratigraphic correlation, isotopic studies and radiometric dating. These “drive” explanation by imposing new demands (adequacy criteria) on would-be explainers; so when it was determined that terrestrial animals experienced a mass extinction near the P–Tr boundary, extinction models that explained only the marine extinction no longer sufficed. Non-explanatory work typically aims at *characterizing* complex phenomena, and the characterization of phenomena is an ongoing process involving the progressive disclosure and articulation of more and more their features (Dresow and Love in preparation). Like bringing an object under a microscope into focus, non-explanatory work brings the features of historical phenomena into focus. And frequently, though not always, this increases the complexity of the explanatory target, requiring researchers to construct more complex explanatory models to meet a growing list of explanatory demands.

The remainder of this section attempts to answer the question posed at the end of the last section: what accounts for the initial complexification and later streamlining of explanatory hypotheses for the end-Permian mass extinction? My answer is organized into two parts of unequal length. In the first and longer part, I consider how developments in a variety of disciplines destabilized the initial “consensus” on the cause(s) of the extinction, and led researchers to develop more complicated models culminating in

Erwin's *Murder on the Orient Express* hypothesis. Then, in the second part, I consider how the further characterization of the extinction led to a streamlining of the causal story, and eventually, to the solidification of a new "consensus" model. In both parts, non-explanatory work will be front and center, just as it is in research into the end-Permian mass extinction.

4.1 From Colliding Continents to "A Fateful Combination of Several Misfortunes," ca. 1970–1993

Recall that early explanations of the end-Permian extinction were geared to explain the disappearance of benthic marine invertebrates, and tended to invoke the loss of marine shelf area as the primary kill mechanism. But as Hallam and Wignall (1997) observe, "this model [was] driven more by a lack of data than by any conclusive evidence, and the picture has become rapidly more complicated as data-gathering has accelerated since the mid-1980s" (94). The opening of China to western geologists was an especially important development, since China contains a greater number of complete P–Tr boundary sections than anywhere else in the world.¹³ These have been critical for dissecting fine-scale extinction patterns, correlating boundary sections around the world, and—most importantly—resolving the rate of the biodiversity crisis.

During the 1970s and 1980s, nearly everyone who worked on the end-Permian mass extinction regarded it as a prolonged affair, beginning with the retreat of the oceans

¹³ China has a rich tradition of paleontological research in the descriptive tradition, and Chinese geologists had performed extensive work on P–Tr boundary sections prior to the warming of Chinese-western relations (Erwin, personal communication). To say that China was "opened to western geologists" is therefore not to imply that the majority of knowledge of Chinese boundary sections was generated by western scientists.

at the end of the Guadalupian epoch (the chunk of time spanning the middle of the Permian Period) and proceeding with accelerating pace into the earliest Triassic.¹⁴ This belief is now extinct. Two studies published in 1994 were especially important in changing researchers' attitudes regarding the rate of the extinction. The first, coauthored by Steven Stanley and Yang Xingling, aimed to determine whether the apparently lengthy extinction at the end of the Permian was actually the result of an imperfect fossil record. Stanley and Yang were interested in whether the high rates of extinction recorded for the Guadalupian epoch were real, or whether they were produced by artificial range truncations (since the last known fossil of a particular taxon is unlikely to correspond to the last living member of that taxon). They devised three tests of the fossil record, which collectively indicated that high Guadalupian extinction rates were not artifacts of an imperfect record—the end-Guadalupian extinction was real and “occurred as a brief pulse, at or near the end of the age” (Stanley and Yang 1994, 1341). Since the end-Permian mass extinction also occurred as a brief pulse, it follows that “Late Permian faunas experienced a *double mass extinction*”—a one-two punch of biological devastation, with five to ten million years of relative calm in between (ibid., 1340).¹⁵ This conclusion was reinforced by a second study based on the rich Chinese fossil record, which also identified extinction spikes at the end of the Guadalupian and at the P–Tr boundary (Jin et al. 1994).

¹⁴ This statement should perhaps be qualified to exclude Chinese geologists, whose views on the extinction were not widely known among western geologists prior to the 1990s (most articles and monographs were published in Chinese or Russian).

¹⁵ The end-Guadalupian mass extinction was not as severe as the end-Permian extinction, which continued to rank as “the most severe biotic crisis of all time” (Stanley and Yang 1994, 1340).

These studies were performed by paleontologists: not for the purpose of testing a particular explanatory claim, but for the purpose of characterizing the temporal structure (duration and rate) of the extinction itself. Yet the task of articulating a temporal framework for the end-Permian mass extinction is not primarily a task for paleontologists. Instead, it is a task for stratigraphers and geochronologists, who seek to ascertain the relative order and absolute timing of events in the rock record, respectively. For much of the twentieth century, studies of the end-Permian extinction were hindered by the lack of a well-constrained temporal framework for understanding the event (Hallam and Wignall 1997). The age of the P–Tr boundary, the relationship between boundary sections in different parts of the world, and the completeness of boundary sections were all objects of considerable uncertainty (Erwin 1993; Hallam and Wignall 1997). Yet things began to change in the 1980s, with the development of a new biostratigraphy based on the ubiquitous marine fossils known as conodonts (marine animals that left behind tooth-like jaw elements in great abundance). For a hundred years before this, the P–Tr boundary was recognized by the first occurrence of the ammonoid *Otoceras woodwardi*, or sparing this, the first appearance of the bivalve genus *Claraia* (Erwin 1993). But *Otoceras* faunas lived only in moderate to high latitudes, whereas the majority of high-quality boundary sections were deposited in low latitudes (Hallam and Wignall 1997). This made correlating boundary sections difficult, and frustrated the project of articulating a high-resolution biostratigraphic framework for understanding patterns of extinction and survival.

Happily, conodonts permit a much finer subdivision of boundary sections than ammonoids, and in 1997, the Permian Subcommittee of the International Union of

Geological Sciences introduced a high-resolution framework in which conodont zones are used to define stage boundaries (Figure 7). This resolved many persistent correlation problems, and produced, for the first time, “a detailed picture of events during the Permian-Triassic (P–Tr) transition” (Erwin et al. 2002, 364). Conodont biostratigraphy indicated, for example, that there are numerous complete boundary sections in the tropical Tethys region, an important biogeographical province that had previously been regarded as lacking complete boundary sections (Wignall 1996).¹⁶ Still, conodont biostratigraphy was hardly a panacea, and important problems remained after the adoption of the improved framework. An especially acute one concerned the correlation between terrestrial and marine rocks.¹⁷ After early indications that terrestrial vertebrates may have avoided a mass extinction near the P–Tr boundary, evidence for a mass extinction had begun to accumulate during the 1980s (e.g., King 1991). Still, evidence for a *correlative* marine and terrestrial mass extinction remained sparse, with many interpreting the terrestrial extinction as a gradual one beginning in the Late Permian and continuing into the Early Triassic. This raised the possibility that distinct kill mechanisms may have operated in the marine and the terrestrial realms—a catastrophic one in the seas and a slower one on land. But if this were the case, then researchers faced the problem of determining how these mechanisms were related to one another. Erwin (1993, 1994), for example, postulated that increased oxidation of organic matter (from the marine

¹⁶ The absence of complete boundary sections was in turn taken to indicate that the Tethys region had experienced a major regression, or sea-level drop, near the P–Tr boundary.

¹⁷ Conodonts are exclusively marine animals. They are therefore not useful for correlating (establishing a temporal correspondence) between marine and terrestrial rocks, except in rare cases where marine and terrestrial rocks interfinger.

regression) led to climatic instability by raising levels of atmospheric CO₂, with the subsequent transgression delivering the *coup de grâce* to land animals by destroying near-shore terrestrial habitats. But this was simply conjecture, however plausible in light of the available evidence. It was all very complicated.

AGE (Ma)			STAGES	CONODONT ZONES	
250	TRIASSIC	Scythian	Anisian	Spathian	<i>Neospathodus timorensis</i>
					<i>N. homeri</i>
					<i>N. collinsoni</i>
					<i>N. triangularis</i>
			Olenekian	Smithian	<i>N. waageni</i>
					<i>N. pakistanensis</i>
			Nammalian	Dienerian	<i>N. cristagalli</i>
					<i>N. dieneri</i>
					<i>N. kummeli</i>
			Induan	Griesbachian	<i>Clarkina carinata</i>
					<i>Isarcicella isarcica</i>
					<i>Hindeodus parvus</i>
			Changxingian (Dorashamian)		<i>C. changxingensis/</i> <i>H. latidentatus</i>
					<i>C. subcarinata</i>
					<i>C. orientalis</i>
260	PERMIAN	Lopingian	Wujiapingian (Dzhulfian)		<i>C. guangyuanensis</i>
					<i>C. leveni</i>
					<i>C. asymmetrica</i>
					<i>C. dukuoensis</i>
					<i>C. postbitteri</i>
					<i>Mesogondolella xuanhanensis</i>
			Maokouan		<i>M. prexuanhuanensis</i>
					<i>M. altudaensis</i>
					<i>M. posteserrata</i>
					<i>M. aserrata</i>
					<i>M. serrata</i>
			Chihsian		<i>Sweetognathus iranicus</i>
					<i>Iranognathus sp.</i>
					<i>M. idahoensis</i>
					<i>Neostreptognathodus pequopenis</i>
			Yanghsingian		
			Leonardian		
			Guadalupian		
			Kazanian		
			Tatarian		
			Ochoan		

Figure 7 A stratigraphic framework for the mid-Permian to Early Triassic interval, showing conodont zones in the right-hand column and traditional stage-level divisions in the center. Notice that conodont zones permit a relatively fine division of the geological

column—a prerequisite for characterizing patterns of extinction and survival at a high level of resolution. (From Hallam and Wignall 1997)

New evidence from geochemistry complicated the picture even more.

Fluctuations in the ratio of “light” to “heavy” isotopes in sedimentary successions reflect changes in ocean chemistry, “and, through a complex network of feedback loops, [changes in] climate, biological abundance, atmospheric composition, continental weathering, and ocean circulation” (Erwin 1993, 186). Isotope studies are therefore a means of determining the geological setting of events in the fossil record, and the Late Permian saw some of the largest isotopic shifts in the past 500 million years (Holser et al. 1987, 1989). Carbon isotopes in particular underwent a dramatic swing, indicating that a huge amount of “light” (organic) carbon was added to the oceans and atmosphere around the P–Tr boundary (Holser et al. 1991).¹⁸ The cause of the swing was difficult to determine, and uncertainty lingered about whether the swing came just before or just after the extinction (and therefore whether it represented the cause of the extinction or its effect). Yet the crucial point is that, once it was known, the carbon swing could not be ignored in explanatory models of the extinction. A massive perturbation of the carbon cycle around the time of the largest mass extinction in history is not something that can be simply set aside. As such, a major focus of extinction models since the nineties has been to explain the aberrant behavior of the carbon cycle; and as Erwin (2006b) notes, “many of the disagreements over the cause of the mass extinction are really a disagreement over the cause of [the] shift in carbon isotopes” (51).

¹⁸ This shift is evidently global—a fact that conodont biostratigraphy helped greatly to establish (see Erwin 1993, Ch. 7). Because of this, the carbon swing can be used to mark the P–Tr boundary; something that is especially useful in sections that lack fossils.

Carbon isotopes are just one geochemical witness to the events of the Late Permian. Isotopes of oxygen, sulfur and strontium also provide clues regarding features of the physical environment—features like climate, volcanism and tectonic activity. Like the carbon isotopic record, the record of other isotopes is not always easy to read; Erwin (2006b) calls the evidence from stable isotopes “abundant, consistent and ambiguous” (185). Yet it is undoubtedly important, and already in the early nineties, studies of oxygen and sulfur had generated some key insights, like the probable stagnation of ocean waters during the Late Permian (Holser et al. 1991). This insight was not exactly new—sedimentological evidence like the existence of black shales had earlier pointed to the probable stagnation of the Permian oceans (Rhodes 1967). Yet isotopic evidence provided an important check on (sometimes ambiguous) sedimentological evidence, and in the end, provided investigators with more to explain than had previously been the case. Figure 6 represents several isotopic shifts, which have now become explanatory targets in their own right. (Figure 5 does not represent these shifts, but was also constructed, in part, to explain the evidence from stable isotopes.)

This narrative is far from comprehensive; but already it is apparent that two decades of non-explanatory work transformed the target of explanation in a number of ways:

1. *Temporal anatomy*. Instead of one drawn-out extinction, research in paleontology and stratigraphy disclosed “a considerably more complex event involving an initial [end-Guadalupian] extinction separated by a period of radiation in the latest

Permian before the wholesale slaughter at the end of the Permian” (Wignall and Twitchett 1996, 1155).

2. *Rate*. Instead of a ten million year extinction event, research in paleontology, stratigraphy and geochronology resolved the duration of the end-Permian mass extinction to less than five million years, and possibly less than three million years (Erwin 1994). (The end-Guadalupian extinction was believed to be similarly brief, but this conclusion was based on more limited data.)
3. *Extinction patterns*. Instead of an indiscriminate extermination of benthic marine invertebrates, research in paleontology and stratigraphy disclosed a “complex [pattern of extinction and survival], with some clades disappearing right before the boundary, others diversifying right up to the boundary, and still others seemingly oblivious to [the] extinction” (Erwin 1994, 231). On land, the picture of extinction patterns also “changed dramatically,” with new research “confirm[ing] the presence of a major end-Permian mass extinction” (Hallam and Wignall 1997, 110).
4. *Geological context*. Finally, research in geochemistry disclosed an increasingly rich picture of the environmental context of the extinction, including major disruptions to several geochemical cycles, whose causes and mutual relationships remained to be determined.

Altogether, this made for a more multifaceted explanatory target than researchers confronted during the 1970s, and one that multiplied demands on would-be explainers. In response, researchers devised more complex explanatory models—models that could

explain complex extinction patterns in the sea *and* on land, and that could account for the “abundant, consistent and ambiguous” evidence from stable isotopes (Erwin 2006b, 185). The most complex hypothesis was also the most authoritative: Erwin’s *Murder on the Orient Express Hypothesis*. This was not the end of the story, however, and in the decades following the mid-1990s, further non-explanatory work would lead to a considerable streamlining of extinction models for the end-Permian, while growing the roster of explanatory resources.

4.2 *Building a New Consensus, 1993–Present*

Since the younger Pliny described “a black and menacing cloud, split by twisted and quivering flashes of fiery breath,” volcanoes have been a symbol of environmental calamity (Walsh 2006, 148). It is appropriate, then, that the greatest environmental calamity in earth’s history is associated with one of the largest episodes of sustained volcanism since the planet’s fiery youth: the eruptions of the Siberian Trap basalts about 250 million years ago.¹⁹

Since geologists are wary of coincidences (and since coincidence in time is an important way of inferring causation in geohistory), one might think that volcanism would always have topped the list of suspects for the end-Permian debacle. Yet early

¹⁹ Flood basalt eruptions are not like the eruption of Vesuvius that Pliny witnessed—an explosive event that blasted rock, ash and volcanic gasses high into the stratosphere. In a flood basalt eruption, runny lavas ooze out of fissures in the ground to form large sheets of igneous rock called basalt. The Siberian eruptions were particularly profligate of basalt, exuding enough lava to cover an area as large as the United States up to a kilometer deep (Erwin 2006b). Evidently there were Vesuvius-style (“pyroclastic”) eruptions happening at the time too: a nasty combination (Wignall 2015).

attempts to date the Siberian flood basalts produced an extraordinarily wide range of ages, from about 280 to 160 Ma (Benton and Twitchett 2003). “According to these ranges, geologists [before] 1990 could conclude only that the basalts might be anything from Early Permian to Late Jurassic in age, but probably spanned the [P–Tr] boundary” (Benton and Twitchett 2003, 361). Merely spanning the boundary, however, was not enough to implicate volcanism in the extinction, especially if the major volcanic episodes happened well before or after the extinction pulse at the P–Tr boundary (as many geologists believed they did). For this reason, most extinction models elaborated before the 1990s ascribed to volcanism “a relatively minor role in the [P–Tr] crisis” (see Figure 5)

But more on this in a moment. In his 1993 book, Douglas Erwin referred to the question of rate as “[p]erhaps the most critical question in evaluating... various extinction mechanisms” (Erwin 1993, 225). The reason is that different kill mechanisms operate on different characteristic time scales, from the very slow (e.g., marine regression, climate change) to the very fast (e.g., bolide impact, volcanic winter). The discovery that the end-Permian extinction had been fast, unfolding over a period of less than three million years, dealt a heavy blow to the original “consensus” model, which relied upon the formation of Pangea and the reduction of shallow sea habitat to bring about the (marine) extinctions. Still, it remained unclear in the early nineties just *how* fast the extinction had been. Erwin’s *Murder on the Orient Express* hypothesis required several million years to operate; anything faster would “[suggest] a single trigger,” as opposed to the patchwork of processes operating on a range of characteristic time scales (Erwin 2006b, 190). The model was therefore damaged by the discovery, in 1998, that the carbon shift took place

over a span of 165,000 years or less, “suggesting a catastrophic addition of light carbon [to the global ocean]” (Bowring et al. 1998, 1039). It was further damaged when Samuel Bowring and colleagues constrained the duration of the extinction to less than 200,000 years (Shen et al. 2011), and then, in 2014, to a mere 60,000 years (plus or minus an uncertainty of 48,000 years) (Burgess et al. 2014). These studies additionally showed that the marine and terrestrial extinctions were effectively simultaneous (within the margin of error of the dating method), and that the surge of light carbon took place over a 10,000-year period just *before* the extinction, eliminating the possibility that the carbon swing represents the effect of the extinction as opposed to its cause. Here, then, is an instance where non-explanatory work served to lighten an explanatory burden, since a simultaneous land and sea extinction is a simpler explanatory target than an out-of-sync extinction unfolding at different rates on land and in the sea.

Which brings us back to volcanoes. Russian geologists first suggested that Siberian Trap volcanism might be implicated in the end-Permian mass extinction during the 1980s, when the age of the Siberian Trap basalts was little known (Benton and Twitchett 2003). Still, the broader geoscience community did not take notice until 1992, when a joint team of Russian and American geologists reported a radiometric date of 248 Ma (± 4 m.y.) for a volcanic intrusion cutting through the flood basalts in Noril’sk in Northern Siberia (Erwin 2006b). Since intrusions must form after the rocks they intrude through, this constrained the age of the Siberian Traps to $>\sim 248$ Ma—*younger*, that is, than the P–Tr boundary. Subsequent work by Paul Renne and colleagues dated the Siberian flood basalts to 250 Ma (± 1.6 m.y.) and the P–Tr boundary to 251.2 Ma. (± 3 m.y.): a degree of propinquity that is hard to ignore (Renne et al. 1995). More recent

dating using improved techniques has tightened the correlation still further. It is now thought that the Siberian flood basalts began erupting ~300,000 years before the extinction, and that they continued erupting for ~500,000 years into the Early Triassic (Burgess and Bowring 2015). It has even been proposed that a particular episode of volcanism—corresponding to a change from lava eruptions to sill complex formation (lateral intrusion of lava into adjacent sediments)—triggered the extinction and the carbon shift by liberating “vast amounts of greenhouse gases” from underground coal reserves (Burgess et al. 2017, 3). If this is true, it represents a considerable streamlining of the extinction model indeed.

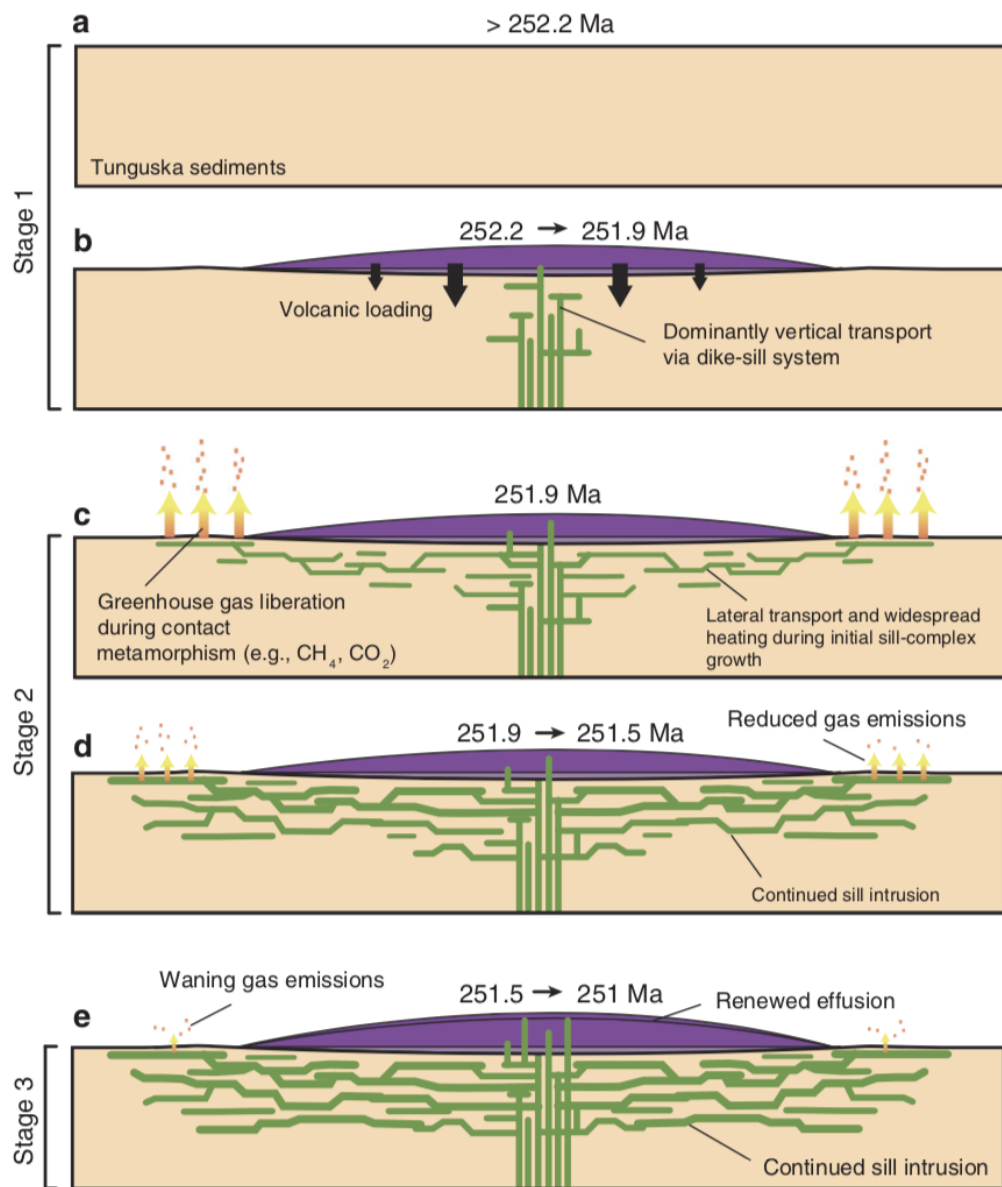


Figure 8 A time series for the abrupt change in emplacement style of the Siberian Trap basalts during the Late Permian, from lava eruptions involving the vertical movement of lava (1b) to sill complex formation involving the lateral movement of lava into adjacent sediments (1c–e). This lateral movement is proposed to have triggered the extinction by liberating vast amounts of greenhouse gasses, depressing the accelerator on the “runaway greenhouse” that ultimately wreaked havoc on marine and terrestrial ecosystems. (From Burgess et al. 2017)

This is not all that has happened since 1993. In a remarkable *volte-face*, “the most favoured cause” of the end-Permian debacle prior to the 1990s—and a popular explanation of the carbon shift—proved to be false (Hallam and Wignall 1997, 132). Sea-levels were not *falling* in the Late Permian, squeezing out marine invertebrates and ramping up the oxidation of organic matter on the continental shelf. Instead, they were *rising*, as detailed fieldwork using the new conodont biostratigraphy made clear (Wignall and Hallam 1992, 1993). Since multiple lines of evidence suggest widespread ocean stagnation in the latest Permian, rising seas may have washed the continental shelves with anoxic water, suffocating any organisms that could not escape the oxygen minima (Wignall and Twitchett 1996).²⁰ Stir in evidence of warming associated with flood basalt eruptions and you have the makings of a new model: one in which volcanism triggers runaway global warming and acid rainfall that (1) denudes the land, (2) increases nutrient delivery to the oceans and (3) promotes the shoaling of anoxic and euxinic water on the continental shelf (Figure 6).²¹ Terrestrial animals likely succumbed to extreme heat and drought associated with warming, or else were snuffed out by the effects of acid rain on

²⁰ In any event, it is clear from new techniques for studying ancient redox conditions that anoxia was widespread during the Late Permian, especially in shallow waters (Feng and Algeo 2014; Clarkson et al. 2016). But here too, the picture is fast becoming more complicated. Instead of a gradual spread of anoxic conditions, the inferred pattern of redox changes now suggests a complex spatial and temporal dynamics, whose relationship to “both evolutionary dynamics and the global carbon cycle” remains unresolved (Clarkson et al. 2016, 2). Resolving these relationships will be critical to filling out new models for the end-Permian extinction (see, e.g., Schobben 2020).

²¹ Euxinia is a condition whereby hydrogen sulfide (H₂S) accumulates in oxygen-free waters, which can lead to deadly hydrogen sulfide poisoning or to hypercapnia (a buildup of carbon dioxide in biological tissues, which can be lethal).

plant life (Benton and Newell 2014; Benton 2018).²² Marine animals either suffocated or were poisoned, or else saw their living space eliminated when anoxic waters from the deep sea intersected with lethal temperatures from the surface (Knoll et al. 2007; Song et al. 2014; Jurikova et al. 2020).

This narrative has grown quite long; still, I have only scratched the surface of the non-explanatory work that has overwhelmingly driven explanations of the end-Permian extinction since the mid-1990s. As ever, attempts to characterize the phenomenon itself have been central (using an ever more sophisticated array of techniques and methodological approaches). Yet research into associated phenomena, like Siberian Traps volcanism, has also paid explanatory dividends: in this case, by growing the roster of explanatory resources upon which explanations of the extinction can draw. It is now possible to say, not only that the extinction is associated with the eruption of the Siberian Trap basalts, but that the extinction seems to have been triggered by a particular episode of volcanism corresponding to “an abrupt change...from dominantly flood lavas to sill intrusions [the lateral transfer of lava into adjacent sediments]” (Burgess et al. 2017, 1). Here we have a case where both the “*explanantia* and *explanandum*” are subject to ongoing processes of investigation,” and where more precisely characterizing the *explanantia* is critical for researchers tasked with hitting a moving target: that is, with explaining a constantly changing *explanandum* (Feest 2017, 1166).

My purpose in this chapter is to explore an enriched picture of the dynamics of explanation in geohistory—one that goes beyond a narrow concern for the testing of

²² There is also evidence for massive wildfires near the P–Tr boundary, which would have contributed to deforestation and, ultimately, to “catastrophic soil erosion” that exacerbated marine anoxia and euxinia (Shen et al. 2011, 1372).

explanatory hypotheses. So far, I have done this mostly by illustration. In the next section, I return to the questions posed at the end of Section 2 to explore what the case of the end-Permian extinction has to teach us about the dynamics of historical explanation in general. I also consider the problem raised at the end of Section 1: what organizes research into complex historical events if not (just) particular explanatory hypotheses?

5. Discussion

5.1 Revisiting the Three Questions

I began this chapter by isolating three questions from Currie's account of historical explanation. These were:

- (1) What accounts for the fact—if it is a fact—that early in the career of an investigative project, many hypotheses are simple, “one-shot hypotheses”?
- (2) What accounts for the fact—if it is a fact—that historical explanations shift from simple to complex as time goes by?
- (3) Is the practice of articulating one-shot hypotheses reasonable given that the world is complex, and therefore unlikely to yield to simple hypotheses?

My criticism of Currie was that he has not yet given satisfactory answers to these questions, in particular (1) and (2). For this reason, his account of historical explanation remains a kinematics—a description of pattern—as opposed to a dynamics. I have indicated in passing what I believe the answers to (1) and (2) to be. In this section, I will flesh these answers out, referring back to the case study to illustrate general points. I will

also comment on question (3): not to supply a direct answer, but to introduce a clarification that changes the scope of the question.

Questions (1) and (2) can be answered together. Early in the career of an investigative project, the features of an explanatory target will tend to be poorly characterized (and some features will not be known at all—think of the carbon shift near the P–Tr boundary). Since the complexity of an explanatory model tends to reflect the constraints on explanation supplied by ongoing characterization, explanatory models formulated during the early stages of an investigative project will tend towards simplicity.²³ Yet as characterization proceeds to supply more constraints on explanation in the form of an expanded roster of adequacy criteria, researchers will be forced to entertain more complex explanations. This trend towards increased complexity is not irrevocable—sometimes, ongoing characterization will *eliminate* constraints on explanation, or disclose a configuration of constraints that favors a simpler explanation of the target phenomenon than an earlier configuration. Still, as ongoing characterization brings more features of historical phenomena into focus, the most typical result—it seems to me—is a multiplication of constraints favoring the development of more complex models. This accounts for Currie’s observation that, in the historical sciences, explanations “shift from simple to complex as time goes by” (1173).²⁴

²³ Whether they will tend to be “one-shot hypotheses” is a separate matter, which I will consider below.

²⁴ This is the “demand-side” of the story, at any rate. The “supply-side” concerns the provision of explanatory resources that support complexification, and here too non-explanatory work is often paramount (see, e.g., Novick et al. 2020 on the role of experiments in furnishing explanatory resources).

The case of the end-Permian mass extinction illustrates this picture nicely. Before the 1970s little was known about the temporal structure of the end-Permian mass extinction, and even its magnitude was in doubt. A prominent review cautioned against “over-dramatizing” it: “Certainly several major taxa of Paleozoic invertebrates became extinct, but the pattern of extinction shows little uniformity...[and] Late Paleozoic extinction rates were by no means unique” (Rhodes 1967, 57, 72). Land animals in particular were “little reduced,” and plants seem to have experienced “no dramatic episode of extinction” (57). Rhodes singled out two features of the end-Permian extinction as requiring explanation. First, researchers needed to explain “the degree and extent of late Permian and early Triassic extinction[s]” (uncertain though these were). Second, they needed to explain “the time-lag between the disappearance [of Paleozoic taxa] and [their] replacement [in Triassic times]” (71). These explanatory requirements were not terribly onerous, and indeed, Rhodes went so far as to suggest that “we [may] need no ‘special’ explanation [for the end-Permian extinction] over and above the explanation of normal extinction” (71). Few paleontologists seem to have agreed with him; still it is remarkable that, in 1967, a leading paleontologist could suggest that all “special” explanations of the end-Permian mass extinction are superfluous—that nothing in our knowledge of the Late Permian *compels* us to entertain extinction models beyond the “null” model. Never again would the evidential situation supply so few constraints on explanations of the extinction.

Consider, by contrast, the list of “observations [that] must be accommodated within any acceptable model for the extinction” according to Erwin et al. (2002). Few of these “observations,” it should be said, were known prior to 1970.

(1) There is widespread evidence for shallow anoxia and some evidence for deep-water anoxia [...] (2) The δ -13 C excursion occurs in both marine and terrestrial sections, and in the marine sections in China corresponds within precision with the primary extinction horizon [...] (3) The age of the extinction in southern China and the eruption of the major phase of the Siberian flood basalts are coincident within experimental error. (4) The marine extinction occurred in ~500 [thousand years] and during a rise in sea-level, not a regression. (5) There is no evidence for latest Permian glaciation. (6) Evidence suggestive of rapid global warming has been accumulating from Russia, Australia, and possibly South Africa. (7) The increase in fungal spores, indicating a disturbance in terrestrial ecosystems, begins before the marine extinction in many sections [...] (8) Possible, although not yet conclusive, evidence of impact has recently been advanced [...] (9) The early onset of the fungal spike and the deep-sea anoxia suggest the possibility that disruption began on land and in the deep sea before shallow-marine ecosystems were affected. (Erwin et al. 2002, 377)

Is it any mystery that, faced with this growing array of things-to-be-explained, explanatory models grew significantly more complex during the 1980s and 1990s, culminating in Erwin's *Murder on the Orient Express* hypothesis?

I noted above that the trend towards increased complexity is not irreversible, and that explanations of the end-Permian extinction constitute one instance of simplification following complexification. The reasons for this were twofold. First, certain constraints on explanation were eliminated—researchers now regard evidence of impact as highly dubious, for example, eliminating the need to explain this evidence away. (No extinction model attempts to account for the presence of shocked quartz near the P–Tr boundary, because there doesn't seem to be any!) Second, a more exact timeline for the extinction and associated phenomena prompted a streamlining of explanatory models; so, for instance, the discovery that the marine and terrestrial extinctions were virtually simultaneous removed the need to explain discrepancies in the timing and rates of the marine and terrestrial extinctions. Likewise, the discovery that the extinction took place

over <100,000 years pointed strongly to a single trigger, damaging hypotheses like the *Murder on the Orient Express* hypothesis, which posited several causal drivers operating on different time scales (see Erwin 2006b).

So much for the first two questions. What about the third? Recall that a one-shot hypothesis is an explanation that researchers treat as mutually-exclusive with other hypotheses, and that “takes a complex history and accounts for it with a single [causal] factor” (2). This makes it sound as if one-shot hypotheses adduce only a single *causal process*, and indeed, some of Currie’s examples conform to this model. (Maybe sauropods became gigantic because an increase in atmospheric oxygen enabled them to grow bigger, full stop.) Yet other of his examples consist of a single triggering event with multiple downstream sequelae, like the impact hypothesis for the K–Pg extinction (Alvarez 1997). In this hypothesis, a bolide impact (the “trigger”) sets massive tidal waves rolling, sparks wildfires, and infuses the stratosphere with aerosols that reduce incoming sunlight, denude the land and acidify the oceans (“kill mechanisms”). Here the geometry of causation resembles a branching tree as opposed to a straight line connecting a discrete cause with its effect. It indicates that some one-shot hypotheses can be indefinitely complex, belying Currie’s claim that one-shot hypotheses are invariably simple (see Figure 6).

Why does this matter? It matters because if one-shot hypotheses are not invariably simple, then no special difficulty attaches to their justification in a complex world.²⁵ In

²⁵ There remains a difficulty with justifying *simple* one-shot hypotheses for complex events, so Currie’s project in his (2019b) is not misguided. He just errs in saying that a special difficulty attaches to the justification of one-shot hypotheses if these are defined to include all explanations (simple or complex) featuring a single trigger.

addition, there is no reason to think that one-shot hypotheses should be confined to the early stages of investigative projects (i.e., that they will be mostly absent from the latter stages of an investigation). Perhaps Currie is right that monocausal hypotheses will be disproportionately represented at early stages of investigation—these hypotheses, which “[take] a complex history and [account] for it with a single [causal process],” *really are* simple, and may be most viable when the evidential situation is most impoverished. Yet other one-shot hypotheses, like those postulating a single trigger and multiple downstream sequelae, can suffice at many junctures in an investigative project, since they can exhibit considerable internal complexity and, for this reason, can satisfy even complex sets of adequacy criteria. Whatever the case, there is nothing anomalous about the most recent “consensus” model for the end-Permian mass extinction, which is a “one-shot hypothesis” by Currie’s definition, albeit a fairly complex one.

5.2 *What Organizes Research into Complex Historical Events?*

There remains the question of what organizes research into complex historical events if not the imperative to confirm or disconfirm explanatory hypotheses. Here I would like to offer a particular and a general answer. The particular answer, for the case of the end-Permian mass extinction, is *features of the event itself* (as represented in extinction models and more general frameworks for understanding the event).²⁶ It is these features that provide the basic organizational scaffolding for ongoing research; so for

²⁶ By “features of the event,” I have in mind things like its temporal structure (what happened, when and for how long), patterns of extinction and survival, and elements of its geological context, including the behavior of earth systems and the environmental effects of massive volcanism.

example, sustained interest in the temporal structure of the extinction has resulted in a decades-long cooperative project aimed at articulating a high-resolution time scale for the extinction and associated phenomena (like Siberian Trap volcanism). Yet the features of complex historical events are not always transparent, especially in the early stages of an investigation. An event like the end-Permian extinction is an “epistemically blurry object of research” in the language of Feest (2017). It is an object whose nature and exact contours are unclear (or at least not as clear as researchers would like them to be). For this reason, a great deal of research is dedicated to providing a better descriptive understanding of the event itself; and since it is features of the event that provide the main organizational scaffolding for research in this area, a recursive cycle arises in which a more articulated understanding of the extinction multiplies the number and variety of research tasks, including tasks that are relevant to the evaluation of explanatory models.

The wider relevance of these observations can be made clear by turning to the general answer. Broadly speaking, research into complex historical events can be understood in terms of lists of interrelated research questions, or things to be done: structured problem agendas (Love 2008, 2014).²⁷ These tasks include both characterization and explanation (Love 2015); so to take the example of the carbon shift, a characterizational task is to constrain the magnitude, timing and extent of the shift, and

²⁷ Currie makes a similar suggestion in his (2019a). Specifically, he suggests that research into mass extinctions can be analyzed as a problem agenda whose “central concerns involve characterizing, finding evidence of, and explaining both particular events in the fossil record, and patterns across it” (7). A difference between Currie’s analysis and mine is that Currie seems to regard the problem agenda of mass extinctions as primarily a paleobiological problem agenda, whereas I regard paleobiology as just one of many disciplinary specialisms involved in a joint project of investigating mass extinctions.

an explanatory task is to explain the source of the isotopically light carbon. Crucially, these tasks are related—the explanation of the carbon shift will be different depending on whether it is a global or merely a regional shift, or whether it came before or after the extinction. But in any event, all tasks must be addressed if the extinction is to be understood in all its multidimensional complexity. Further, since the tasks involve different features of the event (e.g., temporal, biological, geochemical), a range of disciplinary specialisms must be recruited to tackle the various components of the agenda.

This emphasis on diverse specialisms suggests an alternative account of how scientists locate relevant evidence. Recall from Section 1 the argument that most historical research is oriented around the testing of explanatory hypotheses (e.g., Currie and Sterelny 2017). Since historical events are massively underdetermined (the thought goes), and since historical sciences lack general theories to structure inquiry, historical scientists require explanatory hypotheses to locate evidence and overcome underdetermination. But historical scientists are by no means lost in the wilderness without explanatory hypotheses to guide them.²⁸ For one thing, disciplinary specialists can enlist a range of protocols for locating evidence independent of explanatory hypotheses. Geochemists, for example, have protocols for locating and analyzing data on ocean geochemistry that can be used to reconstruct carbon cycle dynamics during the late Permian (see Jurikova et al. 2020). This research addresses a component of the P–Tr problem agenda, but because of its explicit disciplinary province, no problem of locating

²⁸ This does not imply that explanatory hypotheses never provide important scaffolding for locating relevant evidence. What I deny is simply that explanatory hypotheses are required to fulfill this role.

relevant evidence arises. It should also be observed that many explanatory hypotheses emerge from efforts to characterize particular features the past, as opposed to the other way around. For example, Paul Wignall and Anthony Hallam arrived at their model for the end-Permian extinction by the application of an approach that Hallam had long used to study sea-level changes during the Phanerozoic (e.g., Hallam 1981, 1984).²⁹ Now, it is true that Wignall and Hallam had “[a] hunch that marine anoxia was at least heavily implicated in the marine extinctions,” since evidence for marine anoxia near the P–Tr boundary was well-known at the time (Hallam 2004, 121). Still, to claim that an explanatory hypothesis provided critical scaffolding for their research is at best half-true. Here the hunch provided a pointer, but the location of relevant evidence owed more to the procedures for identifying evidence embedded in the broad methodological approach.

Where does this leave hypothesis-testing? Certainly I do not wish to deny that historical scientists test hypotheses—as I noted in Section 1, some research activities in geohistory really *are* designed to put explanatory statements to the test. My position is rather that most research activities in geohistory are not of this sort (they are non-explanatory work). So accounts of historical reconstruction that put hypothesis-testing front and center paint a distorted picture of research in the area.³⁰ Or at least they present

²⁹ The methodological approach involved detailed stratigraphic and facies analysis (Wignall and Hallam 1992, 1993). The model involved rising seas and the onshore movement of anoxic water (which Hallam and Wignall 1997 argue can be found in *every* mass extinction event).

³⁰ Several recent studies of the experimental sciences have reached similar conclusions. For example, O’Malley, Elliott and Burian (2010) have argued that research into microRNA is best characterized in terms of an “iterative movement” between different types of investigation that involves “not only the proposal and testing of hypotheses but also exploratory, technology-oriented and question-driven modes of research” (407). Fagan (2011) argues that in stem cell biology research aimed at testing explicit

a distorted picture in proportion as they cleave to a standard view of hypothesis-testing in which research activities are designed to produce evidence that either confirms or disconfirms explanatory statements. This is distorted since it implies that most research activities in geohistory are oriented towards explanation, while in fact most activities aim to generate a better descriptive understanding of a subject domain. Further, while descriptive understanding is invaluable for constructing and evaluating explanatory models, this should not be taken to imply that apparently descriptive research is in fact explanatory research in disguise.³¹ Even if researchers share an interest in constructing and adjudicating explanatory models, “much of their empirical work is directed toward figuring out what they are actually investigating” (Feest 2017, 1167).³² And this “figuring out” is a matter of characterization, not explanation (Colaço 2020; Dresow and Love in preparation).

hypotheses is “not the whole story,” and suggests that philosophers pay attention to the various activities involved in the construction of models, as well as to the way in which “the structure of models...reflects the organization of their respective communities” (259). Finally, Feest (2017) suggests that philosophers’ preoccupation with explanation has led them to overlook the fact that “objects of research” in the cognitive and behavioral sciences are seldom clearly delineated, and claims that “the research process is better analyzed as one that tries to construct adequate descriptions of epistemically blurry objects of research” (1175).

³¹ A reviewer points out that this claim is sensitive to how we delineate explanatory research. For example, a study with descriptive aims might nonetheless be regarded as “explanatory” if it makes a significant contribution to an explanatory project (adding or subtracting constraints on explanation, say). This strikes me as a perfectly sensible thing to say, and fully compatible with my thesis much “explanatory” research (in this sense) has proximate aims that are unrelated to the evaluation of explanatory hypotheses.

³² Feest is here talking about the cognitive and behavioral sciences, but her point applies with equal force to the sciences of geohistory.

There is a broader view of “hypothesis-testing” that one might adopt, which retains a central place for explanatory hypotheses in historical research (Currie, personal communication). According to this view, an explanatory hypothesis includes both a characterization of a phenomenon *and* an explanation of that phenomenon; and since ongoing research can reveal flaws in either the characterization or the explanation, much (or all) research in geohistory is really hypothesis-testing. This view, I should say, runs together two different kinds of hypotheses: (1) hypotheses about the phenomenon itself, or *descriptive hypotheses*, and (2) hypotheses about the cause of the phenomenon (*explanatory hypotheses* proper). Yet this is not my complaint. My complaint is that this view implies that all characterizational activities are attempts to test *some component* of an explanatory hypothesis, and this is at variance with how researchers in geohistory understand their own research. A stratigrapher engaged in correlating P–Tr boundary sections is not attempting to test an explanatory hypothesis; rather, she is trying to unravel the sequence of formations spanning the P–Tr boundary. Hence, even if her results may be relevant to the evaluation of an explanatory hypothesis, it seems perverse to describe her as engaged in a test of that hypothesis. Likewise, a paleontologist interested in characterizing patterns of extinction in gastropods needn’t be doing so with a particular hypothesis in mind: it might be genuinely exploratory research. But assuming that his research bears positively or negatively on an explanatory hypothesis, does it then become (retrospectively) a test of this hypothesis? Only adventitiously. The research remains exploratory in conception.

Philosophers have a well-known penchant for explanation, and for activities involved in explanatory reasoning, like hypothesis-testing (Waters 2004). Yet in

geohistorical research (as elsewhere in the sciences), explanation and hypothesis-testing are only part of the story (O'Malley et al. 2010; Fagan 2011; Feest 2017; Bokulich 2018). At least as large a part, I have suggested, is non-explanatory work: work aimed at increasing our descriptive understanding of a phenomenon. It follows that to understand the process of geohistorical research, one must understand how historical inquiry is organized to explore the contours of its research objects, and to bring explanatory resources to bear on complex events. In the language of this chapter, one must understand how different kinds of non-explanatory work function to bring historical events into focus and to embellish the store of explanatory resources. I have suggested that the notion of a structured problem agenda is useful in this connection, since it concerns how the burden of inquiry is distributed over a multidisciplinary research community for the purpose of adequately compassing a research object. The challenge for philosophers interested in explanation is to show how progress on different parts of an agenda is translated into progress in explaining a complex phenomenon. Part of the answer must involve the disclosure of explanatory resources to meet a changing set of adequacy criteria (see, e.g., Currie 2019b). Another part must involve the “increased refinement, articulation and specification of [research] questions with [a problem agenda itself]” (Love 2014, 52). In any event, there remains ample scope for future philosophical discussions of the dynamics of explanation, both within and without the sciences of geohistory.

6. Recapitulation

In this chapter, I explored the dynamics of explanation in geohistory using research into the end-Permian mass extinction as a case study. I argued, first, that a major

“driver” of explanation in geohistory is non-explanatory work—work that is undertaken to increase the descriptive understanding of a phenomenon, not to test a particular explanatory claim. I argued that non-explanatory work drives explanation by imposing (or eliminating) demands on explanation and by furnishing new resources for the construction of explanatory models. The tendency of such work to impose new demands on explanation provides a rationale for Currie’s observation that explanations of “[h]ighly contingent, disunified events” tend to “shift from simple to complex as time goes by” (Currie 2014, 1173). Explanations grow more complex when ongoing characterization multiplies the demands on explanation, prompting researchers to develop more complex explanatory models. Moreover, researchers are able to produce such models because ongoing characterization also grows the roster of explanatory resources from which explanatory models are constructed. Yet the fact that non-explanatory work sometimes *eliminates* demands on explanation means that the trend towards increased complexity is not irrevocable; sometimes ongoing investigation favors the streamlining of hypotheses, as recent research into the end-Permian mass extinction shows. Finally, I suggested that to achieve a better understanding of the dynamics of explanation in geohistory and elsewhere, it is necessary to grapple with the question of how research into complex phenomena is organized. One useful resource for thinking about this question is the notion of a structured problem agenda. A challenge for philosophers is to show how progress on different parts of a problem agenda is translated into progress in explanation; in other words, to show how the process of explanation depends upon the coordination of different kinds of research activities, and ultimately, different kinds of material and epistemic resources.

Chapter 4:

Uniformitarianism Re-examined, or The Present is the Key to the Past, Except When it Isn't (And Even Then it Kind of Is)

1. “The Present is the Key to the Past”

In 1946, a twenty-seven year old geologist named Reginald (Reg) Sprigg stopped for lunch near an abandoned lead-silver mine in Southern Australia. He had been sent to map the area in order to determine whether any valuable metals exist in the vicinity of the old Ediacaran mines, some 300 kilometers north of Adelaide. The hill on which he took his lunch was composed of weathered sandstones, many of them containing “flimsy clay partings” that Sprigg judged to be favorable to the preservation of fossil animals (Sprigg 1988, 49). And as Sprigg inspected the partings, he did indeed find animal-like fossils: faint impressions of shallow disks, which he described in his notebook as “queer markings suggestive of jellyfish.”



Figure 9 Sprigg's "jellyfish," discovered on May 31, 1946 in the quartzite flagstones near the Ediacaran mines, and later named *Ediacaria flindersi*. (From Sprigg 1988)

The significance of these fossils lay in their age: early Cambrian, Sprigg thought, close to the dawn of "visible" life.¹ Paleontologists knew that living things must have existed before this time, even complex multicellular animals like trilobites and jellyfish. But these had evidently left no record in the rocks. Perhaps they were too diminutive to form fossils, or perhaps their tissues were just not amenable to fossilization. Whatever the

¹ The past 541 million years, spanning the earliest Cambrian to the present day, is known as the Phanerozoic Eon, from the Greek *phanerós* and *zōē*, meaning "visible life." The term "Phanerozoic" was coined in the 1930s, at which time little evidence for Precambrian life existed (McMenamin and McMenamin 1990). The period before 541 Ma is known as the Proterozoic Eon (the period of "first life").

case, multicellular organisms seemed confined as if by a law of nature to the period following the earliest Cambrian, when a medley of animals burst explosively into the fossil record. Indeed, when Sprigg returned to Adelaide with his supposed jellyfish in tow, the term “Precambrian paleontology” was virtually a misnomer (Schopf 2001).

This was soon to change, but not right away. To Sprigg’s evident disgust, his colleagues at Adelaide were none too impressed with his findings—not just with the “jellyfish,” but also with several other fossils collected during a second trip to the Ediacaran hills (Turner and Oldroyd 2009). His disgust was compounded when a letter announcing these fossils was rejected by the journal *Nature* (Sprigg 1988). Sprigg did publish two articles describing his fossils in regional journals, and exhibited photographs of the specimens at exhibitions in Europe and North America. Still, it took more than a decade for the paleontological community to take a serious interest in his findings, after which time the field of Precambrian paleontology was off and running.

The reason is that Sprigg’s fossils were older than he initially thought. In a paper of 1947, he had placed them “at the top of the Adelaide Series” (a group of rocks thought to span the uppermost Proterozoic and the lowermost Cambrian), and suggested that they lived at the beginning of the Cambrian Period (Turner and Oldroyd 2009, 260). Yet by the 1960s, it had become evident that this date was too young. Sprigg’s fossils were found in rocks considerably *older* than rocks known to be Cambrian in age, which is to say, rocks containing characteristic Cambrian fossils. And what’s more, fossils resembling Sprigg’s “jellyfish” had begun to turn up in rocks of Precambrian age all over the world. Increasingly, it came to seem as if these fossils might hold the key to understanding the early evolution of life, including its apparently explosive

diversification in the Cambrian Period. There was just one problem. The more Precambrian fossils turned up, the more bizarre the animals (?) of the “Ediacara biota” seemed to have been. There were radial disks that seem to have lain prostrate on the seafloor, bilaterally symmetrical forms lacking clear front and back ends, upright fronds with rigid spines, aptly-named “pizza disks,” and inverted cones that look a bit like Bugles. All told, they formed a motley assemblage, and seemed an unlikely prelude to the familiar drama of animal evolution that unfolded over the next half-billion years.

It is a testament to the power of convention that in the first decade of research into the Ediacaran fossils, nothing much seemed to be amiss (Gould 1989b; Droser et al. 2017). Martin Glaessner, the first paleontologist of international reputation to describe the fossils, classified most specimens as primitive representatives of modern groups—mostly soft corals and jellyfish, but also some annelids and arthropods. This matched an expectation that the “Eo-Cambrian” would be inhabited by worms and jellyfish, as well as other early models of still-existing designs (Sprigg 1988). Yet beginning in the 1980s, the phylogenetic placement of the Ediacaran fossils began to attract serious attention. Whereas Glaessner had emphasized resemblances in two-dimensional structure between Ediacaran and modern forms, new investigations lingered on their differences (Narbonne 1998). These were so considerable that one influential paleontologist, Adolf Seilacher, proposed to accommodate the Ediacarans within a separate *kingdom* of life, the “Vendozoa” (later downgraded to the phylum “Vendobionta”). According to Seilacher, all the so-called “vendobionts” were constructed on a unique body plan, which Stephen Jay Gould describes as “a flattened form divided into sections that are matted or quilted together...like an air mattress” (Gould 1989b, 312). Seilacher additionally suggested that

these organisms lacked both mouths and guts, and that they received their energy by absorbing organic molecules directly from seawater, or else by harboring chemosynthetic symbionts in their tissues. Needless to say, no organisms of this sort exist in the modern oceans, leading Seilacher to conclude that the vendobionts constitute a failed experiment in multicellular life.²

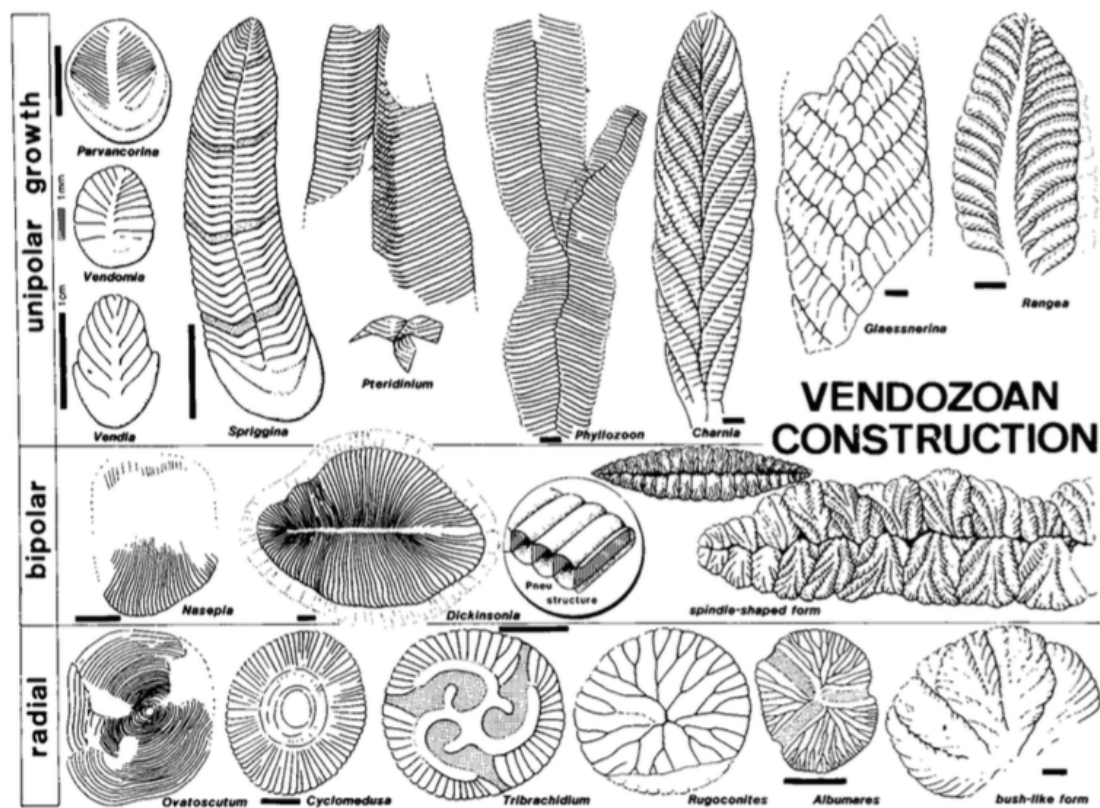


Figure 10 Seilacher's illustration of the Vendozoan body plan, consisting of quilted "pneus," or flexible balloon structures. Notice how very *unlike* jellyfish these creatures seem to be. (From Seilacher 1989)

² It is interesting to note that, by the late 1990s, no undoubted jellyfish remained among the Ediacara biota (Narbonne 1998).

Today it is widely thought that Seilacher went too far. Ediacaran fossils do not form a single extinct clade, nor are they all built on a single unique body plan (see, e.g., Erwin and Valentine 2013, Ch. 5). Still, their phylogenetic affinities and functional morphology remain unresolved. In the words of leading Precambrian paleontologist Andrew Knoll, “Vendobionts provide a Rorschach test for paleontologists. Individual fossils have been interpreted as colonial cnidarians, as segmented worms, as primitive arthropods, seaweeds, lichens, and more [including fungi, giant protists and biological air mattresses]” (Knoll 2003, 167). This diversity of interpretations is rooted in the strangeness of the organisms themselves. Ediacaran fossils “show flashes of familiar biology,” but even their familiar characteristics “occur in decidedly unfamiliar combinations” (Knoll 2003, 173). As a consequence, efforts to reconstruct Ediacaran organisms suffer from an absence of analogs—even partial analogs, in many cases. While it is comparably easy to reconstruct a trilobite, by mapping its features onto those of a woodlouse or a horseshoe crab, say, it is considerably harder to reconstruct an organism whose phylogenetic affinities are unclear and whose characters occur in funny combinations. Here the core dictum of the historical scientist serves only to highlight their predicament. The present may be “the key to the past,” but there is no guarantee that this key will unbolt every door. Sometimes the past is truly alien—a “foreign country,” as L.P. Hartley put it in *The Go-Between*. In such cases, ordinary reasoning strategies are prone to break down, raising the question of what limits exist to our knowledge of deep history.³

³ Recent work on ancient biomolecules has provided a new line of evidence bearing on the phylogenetic affinities of certain Ediacarans. In particular, it now seems probable that

This chapter is about the several senses in which “the present is the key to the past” or fails to be. In other words, it is a chapter about *uniformitarianism*—possibly the most confounded term in the geological lexicon. Uniformitarianism has been taken to mean everything from “laws of nature do not vary in time and space” to “present processes provide the best guide to interpreting the remains of historical events” (see Section 2). Yet despite numerous attempts to analyze its complexity and regiment its use, disagreements persist about what exactly “uniformitarianism” means, as well as the extent to which the present really *is* the key to the past (e.g., Ericksson et al. 1998; Donaldson et al. 2002; Virgili 2007; Erwin 2011; Baker 2014; Knight and Harrison 2014; Romano 2015). This chapter does not aim to resolve these disputes, nor does it aim to settle—once and for all—what the proper understanding of “uniformitarianism” should be. Rather, its goal is to ascertain *what is at stake* in continuing debates about uniformitarianism in geology, and to relate these to the several “forms of understanding” pursued in geohistorical research (see Parker 2014).

The remainder of this chapter is organized in four sections. In Section 2, I provide a brief primer on “uniformitarianism” (sometimes called the “principle of uniformity”), focusing on the term’s history and attempts to regiment its use. In Section 3, I turn to the subject of understanding, and suggest that students of geohistory pursue at least four distinct forms of understanding: (1) understanding what happened; (2) understanding why something happened; (3) understanding complex earth systems; and (4) understanding the geological record itself. Each form of understanding, I claim, is associated with a

Dickinsonia and its relatives are metazoans (animals), although many mysteries remain (Bobrovskiy et. al. 2014; see also Droser and Gehling 2015).

different sense of “uniformitarian[ism]”: uniformitarian[ism] as (1) a reasoning strategy, (2) a kind of explanation (or cause), (3) an assumption about earth system dynamics, and (4) a kind of study, respectively. And each of these is associated with a different substantive question: (1) is uniformitarianism necessary; (2) when is it appropriate to give (non-) uniformitarian explanations of past events; (3) does the evolution of the earth system—including the changes associated with the Anthropocene—render uniformitarianism otiose; and (4) how useful are uniformitarian (or “actualistic”) studies of the geological record itself? Section 4 examines each of these questions in detail, and suggests that it is because they are severally disputed that uniformitarianism continues to occupy an ambiguous place in geological discourse. Finally, in Section 5, I offer a brief perspective on the status of uniformitarianism in the contemporary geosciences, and explain the title of this chapter.

2. Uniformitarianism: A Primer

Perhaps no concept in the geological lexicon excites more passions than *uniformitarianism*, sometimes called “the principle of uniformitarianism” (or just “the principle of uniformity”). On the positive side, it has been called “[the] cornerstone of geologic philosophy” (Longwell and Flint 1955, 385), “the closest thing geologists have to a fundamental ‘law’” (Schoch 1989, 70), “the dominant paradigm in modern geology” (Marriner et al. 2010, 43) and “a fundamental operational principle, without which geology cannot be thought of in scientific terms” (Cloud 1961, 510). Carmina Virgili writes that the principle of uniformitarianism “converts the stratigraphic series [i.e., the vertical stack of rock formations] into an archive of the Earth's history” (Virgili 2007,

576). Likewise, geologist-philosopher David Kitts claims that “[in] terms of the way a geologist operates, there is no past until the assumption of uniformity has been made” (Kitts 1977, 63). Only by assuming that geological processes have operated uniformly through time can we infer what the past must have been like on the basis of surviving material evidence. It follows that uniformitarianism is something like “a condition on the possibility of reconstructing geohistory.” It is an “unprovable postulate” justified on the grounds that—absent this postulate—no “rational interpretation of [earth’s] history [is] possible” (Simpson 1963, 33).

It has not always been so. The term “uniformitarian[ism]” entered geological discourse through an 1832 article published in the *Quarterly Review*. The author was William Whewell, then professor of mineralogy at Trinity College, and later the 15th president of the Geological Society of London. His subject was Charles Lyell’s *Principles of Geology*, which had brought to boil a long-simmering debate about the rate and intensity of geological change. Whewell observed that geologists at the time were divided into two major “sects,” which he named “the *Uniformitarians* and the *Catastrophists*” (Whewell 1832, 125). The former regarded the course of nature as uniform through all geological periods, and “the changes which lead [from] one geological state to another [as], on a long average, uniform in their intensity.”⁴ The latter regarded the history of the earth as directional, and accepted at face value evidence suggesting that the globe is periodically convulsed with major paroxysms. Whewell cast his lot with the catastrophists, as did nearly all geologists in the first half of the nineteenth

⁴ This meant both that the causes of geological change are the same now as they have ever been, and that their effect has been to maintain the earth in a kind of steady-state: the more things change, the more they stay the same.

century (see Rudwick 2008). Yet he did *not* deny that “the present is the key to the past,” which, Whewell observed, had long been the working principle of all competent geologists (see also Sedgwick 1832).⁵ What he denied was that the present teaches us that the course of nature has been uniform through all geological periods. This, Whewell thought, did not square with the geological evidence. Could the astonishing contortions of strata associated with mountain-building really have been produced by small causes acting over vast stretches of time? And what about the abrupt disappearance of whole communities of animals? Whewell was incredulous.

At this point, the story gets messy. As historians have observed, Lyell ran together several senses of “uniformity” in his *Principles of Geology* (of which, more in a moment).⁶ His critics noticed this almost immediately, yet later geologists were less discerning, and continued to treat “the principle of uniformity” as an indivisible whole even as components of Lyell’s system were set aside (Gould 1987b). The most startling claim contained in the *Principles*—that the earth exists in a kind of steady state, with no direction to its history—was never very plausible, and was quickly separated from the rest of Lyell’s doctrine. Lyell himself eventually abandoned this idea, but he did not abandon the idea that “events in the deep past have *never* been of greater extent, suddenness, intensity, or violence...than actual causes” (Rudwick 2008, 304). It was this “doctrine of absolute uniformity” that came to represent his primary contribution to

⁵ In Whewell’s day, this principle was most often associated with an emphasis on *causes actuelles*, or “actual causes” (meaning “causes acting in the observable present,” as opposed to “real” or “authentic” causes). Hence our term “actualism,” commonly regarded as a synonym for “[methodological] uniformitarianism” (see below).

⁶ Lyell does not use the term “uniformitarianism” or any of its variants in *Principles of Geology*.

geological thought, and that later generations would complain placed geological thinking in a straitjacket (Lyell 1830, 87). Still, there was something to uniformitarianism—even critics admitted as much. The question was whether this sane core justified keeping the concept around. For many geologists in the second half of the twentieth century, it did not (e.g., Krynine 1956; Gould 1965; Valentine 1966; Goodman 1967; Shea 1982; Baker 1998). This was typically because they took the core of the concept to be a general statement about scientific method—one that said little more than “geologists should reason scientifically”—as opposed to an indispensable claim about the nature of historical reasoning as such.

What, then, is uniformitarianism? No one thing, to be sure. Stephen Jay Gould, in his first publication, distinguished two senses of the term—the former a claim about scientific procedure (“methodological uniformitarianism”), the latter a claim about how the world works (“substantive uniformitarianism”) (Gould 1965).⁷ Martin Rudwick subsequently distinguished four senses of “uniformity” (as in “the principle of uniformity”), all of which Lyell employed in his *Principles* (Rudwick 1972). These are: (1) *uniformity of law*, which states that natural laws do not vary in space or time, (2) *uniformity of process*, which states that the same geological processes operated now as in the past, (3) *uniformity of rate*, which states that geological processes in the past operated

⁷ More specifically, Gould distinguished between uniformitarianism as “a procedural principle asserting spatial and temporal invariance of natural laws” and what Lyell called *the doctrine of absolute uniformity* (“a testable theory of geologic change postulating uniformity of rates or material conditions,” Gould 1965, 223). Unbeknownst to Gould, a similar distinction had been drawn by a number of nineteenth century geologists, including Whewell, who observed that Lyell’s prohibition against invoking unobservable causes was detachable from his claims about the rate and intensity of geological change (see Rudwick 2008).

at the same intensity as those operating today, and (4) *uniformity of state*, which holds that the earth exists in a kind of dynamic steady-state (see also Gould 1987b, 119–126).⁸

In a similar vein, M. King Hubbert observed that four “common, but not necessarily equivalent” answers may be given to the question “what, precisely, is the Principle of Uniformity” (Hubbert 1967, 4). They are:

1. The present is the key to the past.
2. Former changes of the earth’s surface may be explained by reference to causes now in operation.
3. The history of the earth may be deciphered in terms of present observations, on the assumption that physical and chemical laws are invariant with time.
4. Not only are physical laws uniform, that is invariant with time, but the events of the geologic past have proceeded at an approximately uniform rate, and have involved the same processes as those which occur at present. (Hubbert 1967, 4)

More recent commentaries have mostly recapitulated these distinctions, with a few minor wrinkles (e.g., the use of “strong-” and “weak-uniformitarianism” to mean a combination of substantive and methodological, and methodological uniformitarianism, respectively) (see Balizshov 1994; Knight and Harrison 2014). In addition, it is becoming customary in philosophical circles to distinguish “actualism” (methodological uniformitarianism, broadly construed) from “uniformitarianism” or “gradualism” (see, e.g., Love and Lugar 2013; Currie 2019c). This distinction is a coherent one, yet in the interest of clarity, it

⁸ Claim (1) corresponds to Gould’s “methodological uniformitarianism”; claims (3) and (4) correspond to his “substantive uniformitarianism.” Claim (2) corresponds to the position usually called “actualism,” although actualism is sometimes defined in terms of the uniformity of natural laws (Claim 1), and methodological uniformitarianism is sometimes defined in terms of the uniformity of process (Claim 2).

would probably be best if uniformitarianism *sensu* gradualism were henceforth referred to simply as “gradualism.”⁹

As I stated in Section 1, my aim in this chapter is not to analyze the meaning(s) of “uniformitarianism” as a way of legislating terminological practice. Nor is it to tame “semantic chaos” by tracing the chaos of meanings to a conflation of several senses of “uniformity” in Lyell’s *Principles* or elsewhere (Romano 2015, 66). Rather, my aim is to determine what is at stake in continuing discussions of uniformitarianism in geology, and to analyze why terms like “uniformitarianism” and “uniformitarian” continue to stir strong feelings. To state the obvious, part of the reason indeed traces to ongoing conflation. So, when Ward and Kirschvink write that “uniformitarianism...is outmoded and largely refuted,” it is substantive uniformitarianism they have in mind, yet they draw from this the methodological conclusion that “[the] modern world is not the best tool for explaining many times and events in the deep past” (Ward and Kirschvink 2015, 5). Claims like this arguably trade on a confusion between substantive and methodological uniformitarianism (but see the discussion in Section 4.1 for a different interpretation, especially fn. 15).

Still, conflations like this have become less common in recent decades, and at any rate, the present tensions surrounding uniformitarianism are stickier than those that arise

⁹ Meghan Page (forthcoming) also distinguishes uniformitarianism and actualism, yet for her, actualism involves a commitment to the spatiotemporal invariance of natural laws, whereas uniformitarianism involves a commitment to constant rates of change or material conditions. On this view, Gould’s methodological uniformitarianism ought to be called “methodological actualism.” Methodological uniformitarianism then becomes a procedural assumption asserting the constancy of rates of change or material conditions through time. (Page also recognizes substantive forms of uniformitarianism and actualism.)

out of a simple confusion of meanings. Instead, they arise out of legitimate disagreements over substantive issues, like whether uniformitarian models can be used to understand all environments preserved in the sedimentary record, and how useful actualistic studies of the fossil record really are. Section 4 considers these issues in detail by examining four substantive questions that continue to provoke debate amongst students of geohistory. Before coming to this, however, there remains a bit of table-setting to do. The debate about uniformitarianism is nowadays centered on the issue of how researchers acquire knowledge about the past (substantive uniformitarianism has long been regarded as empirically refuted). But what kinds of understanding do students of geohistory actively pursue?

3. Forms of Understanding in Geohistory

The goal of geohistorical research is to increase our understanding of the past, as well as those records of the past that constitute our main epistemic resource. A more toothless claim about the aims of geohistorical research can hardly be imagined. Still, for all its toothlessness, the claim has a surprising amount of bite. This is because philosophers have not always taken historical research to be about understanding, or if they have, it is a particular *kind* of understanding they have had in mind: the kind conveyed by narrative explanations, say, or by common origin inferences. Forber and Griffith (2011), for example, claim that the goal of historical reconstruction is to craft causal etiologies for events known from the historical record. Likewise, Frodeman (2003) says that historical science is defined by the role that narrative explanations play in its practice. But surely this is too narrow, since many historical researchers are not interested

in *events*, but in past environments and conditions, and one does not explain environments and conditions with causal narratives. Better to say that historical reconstruction is about understanding, and then to scrutinize what this understanding consists in and how it is characteristically achieved.

This section is about the kinds of understanding pursued in geohistorical research—it is not about scientific understanding in general. As such, I will sidestep the ongoing debate about the nature of scientific understanding and follow Parker (2014) in delineating several “forms of understanding” pursued in a domain of research.¹⁰ These will in turn provide a framework for my discussion of uniformitarianism, since each form of understanding raises a different question that bears on the significance or applicability of uniformitarianism in geology. Although Parker is concerned with a different subject domain, I will begin with an overview of her analysis, since the forms of understanding she identifies are also pursued by researchers in geohistory. I will then suggest that students of geohistory pursue two forms of understanding that Parker does not discuss, and provide a brief characterization of each.

Parker’s concern is to articulate the kinds of understanding sought by researchers interested in weather and climate. Based on a review of “classic papers, textbooks and research monographs,” she concludes that “there are at least two [forms] of understanding [sought] in this arena” (Parker 2014, 338). The first she calls *understanding why an event/phenomenon occurs*. This is the sort of understanding

¹⁰ Forms of understanding, in Parker’s sense, can be understood as the types of knowledge in a domain that researchers pursue and value. (So, to identify a form of understanding is to identify a broad epistemic goal harbored by researchers in an area of inquiry.) For discussions of scientific understanding in general, see, e.g., de Regt and Dieks 2005; Potochnik 2017; Khalifa 2017.

associated with having an explanation of an event or phenomenon—one that contains an account of how a set of causal factors (e.g., forces, processes or conditions) interact to produce the focal event or phenomenon. The other is *understanding a complex phenomenon/system*, and is the kind of understanding associated with knowing your way around a complex research object, like “the atmosphere and climate system as a whole” (339). In Parker’s words, this form of understanding involves “both knowledge [that] and [knowledge how], that is, both knowing things about the phenomenon or system...and being able to synthesize and apply that knowledge to answer correctly additional questions about...the effects of interventions on or changes to the system.” She adds that unlike *understanding why an event/phenomenon occurs*, *understanding a complex phenomenon/system* “generally does not have a clear point of completion” (340). Instead, it is achieved only to a greater or lesser extent, when a community “gains significant new knowledge about the phenomenon or system, or refines existing knowledge, or enhances its ability to synthesize and apply existing knowledge to correctly answer additional questions.”

Geohistorical scientists pursue both these forms of understanding. Much high-profile research in recent decades has sought to unravel the causes of major events, like the explosive increase in metazoan diversity that took place near the beginning of the Cambrian Period. This is an example of *understanding why an event/phenomenon occurs*, and there are countless others, from efforts to understand the end-Permian mass extinction to efforts to explain the Paleocene-Eocene thermal maximum. But students of geohistory also want to understand complex systems and phenomena *as such*, especially so-called “geobiologists” and their allies, geochemists and earth system scientists.

Perhaps the central challenge in geobiology is to understand “how organisms influence the physical Earth and vice versa, and how biological and physical processes have interacted through our planet’s long history” (Knoll et al. 2012, 2). This subject is ripe with intrinsic interest, but it is also potentially useful, since understanding the history of the “earth-life system” may help us better understand its future behavior, including its likely response to perturbations associated with anthropogenic climate change. It follows that some geohistorical researchers, at least, are interested in applying their understanding “to answer correctly additional questions about [a] phenomenon or system, especially questions about the effects of interventions on or changes to the system” (Parker 2014, 339).

Yet these forms of understanding do not exhaust the forms of understanding pursued in geohistorical research. In addition to understanding the causes of past events and processes, as well as “how biological and physical processes have interacted through our planet’s long history,” geohistorical scientists are interested in understanding *the geological record itself*, including scales of temporal and spatial resolution and controls on fidelity (e.g., how accurately does a death assemblage reflect its corresponding life assemblage). I regard this as distinct form of understanding: first, because it has a distinct subject matter (it is not about what happened in the past, but rather about how we *know* about what happened in the past), and second, because it is typically pursued by specialist researchers in areas like stratigraphy and taphonomy. Researchers in these areas ask questions like: what is the temporal resolution of the fossil record in different sedimentary environments, and what factors can garble the signal preserved in outcrops and sedimentary basins? And while answers to these questions *do* teach us about events

and processes in the past (for example, they teach us about how organic remains are incorporated into the rock record), the understanding provided is less about particular events and processes as it is about the quality and resolution of historical evidence. For this reason, it is useful to describe it as something separate from an understanding of geohistory *per se*, namely, an understanding of the geological record.

We can also distinguish a fourth form of understanding, which is partially constitutive of—but, I suggest, also distinct from—the first two forms. This might be called *understanding what happened*, or *understanding what the past was like*, and it consists in possessing a more or less detailed characterization of a historical phenomenon (e.g., an event, process, condition or entity). I say that this form of understanding is “partially constitutive” of other forms because *understanding why something happened* and *understanding complex historical systems* requires one to understand *what happened*, including the rates and durations of important processes (see Erwin 2006a). Yet it would be misleading to claim that researchers only care about what happened as a means to the end of giving explanations or characterizing earth-system dynamics. Learning that the end-Permian mass extinction was rapid, taking place over a span of <60,000 years, is a valuable contribution to our understanding of the past, regardless of what it tells us about the causes of the extinction.¹¹ Likewise, determining that a sedimentary structure represents the remains of an ancient cold seep is an achievement, regardless of whether it prompts us to revise our view of earth-system dynamics (Bottjer et al. 1995).¹² (The

¹¹ For instance, it tells us that the extinction was a veritably “catastrophic” one, as opposed to a more drawn-out affair.

¹² There is a confusion lurking here. Many historical inferences are justified on explanatory grounds; we infer that a sedimentary structure *S* is the remains of a cold seep because presupposing that *S* is the remains of a cold seep explains its otherwise puzzling

challenge of reconstructing the Ediacara biota, discussed in Section 1, is also a matter of understanding what the past was like.)

In this section, I have argued that geohistorical scientists are interested in no fewer than four distinct “forms of understanding.” These are: *understanding what happened*, *understanding why something happened*, *understanding complex earth systems* and *understanding the geological record itself*. But what does this have to do with uniformitarianism? Quite a bit, it turns out. In the next section, I argue that each form of understanding pursued by geohistorical researchers raises a different question about the adequacy or applicability of uniformitarianism. The inability to resolve these questions—not mere semantic chaos—is a major reason why uniformitarianism continues to occupy a contested place in the discourse of earth scientists.

4. Four Substantive Questions

This section examines four questions about uniformitarianism raised by the distinct forms of understanding pursued in geohistorical research. Each form of understanding, I claim, is associated with a different sense of the term “uniformitarian[ism].” And each sense of “uniformitarian[ism]” is associated with a different question about the adequacy or applicability of uniformitarianism in the relevant domain (see Table 1).

features. Yet determining that *S* was a cold seep is a matter of characterization as opposed to an explanation of a historical phenomenon. It tells us *what the past was like*, but it does not confer an understanding of *why something happened* (unless that “something” is the formation of *S* itself, but even this requires additional information beyond the knowledge that *S* was a cold seep).

Form of understanding	Focal sense of “uniformitarian[ism]”	Substantive question
<i>Understanding what happened</i>	A reasoning strategy	Is uniformitarianism necessary?
<i>Understanding why something happened</i>	A kind of explanation or cause	When is it appropriate to give (non-) uniformitarian explanations of past events?
<i>Understanding complex earth systems</i>	An assumption about earth system dynamics	Does the evolution of the earth-life system—including the changes associated with the Anthropocene—render uniformitarianism otiose?
<i>Understanding the geological record itself</i>	A kind of study	How useful are uniformitarian (or “actualistic”) studies of the geological record itself?

Table 1 The association of different forms of understanding pursued by geohistorical researchers with (1) a focal sense of “uniformitarian[ism]” and (2) a substantive question about the adequacy or applicability of uniformitarianism in the relevant domain.

1. Is uniformitarianism necessary? (Understanding what happened/what the past was like)

The first substantive question is about uniformitarianism as a reasoning strategy. Specifically, it is a question about uniformitarianism as a means of determining what happened in the past (or what the past was like). The question will be familiar to veterans of the uniformitarianism debate in geology. It is the question that Gould asked in his (1965) and that gave that paper its title. Gould’s answer, in a word, was “no.” Substantive uniformitarianism is a refuted theory of geological change, whereas methodological

uniformitarianism—the postulate that the laws of nature do not vary in space and time—is little more than an affirmation that geology is a science (hardly necessary in 1965). It follows that the term uniformitarianism can be confined to the history books. This eliminativist line continues to find advocates among practicing geologists (see, for example, Shea 1982; Baker 1998; Romano 2015).

In my view, the question “is uniformitarianism necessary?” actually contains two questions. The first asks whether uniformitarianism is redundant: does the principle of uniformitarianism simply paraphrase a more general principle of scientific reasoning, like the principle of parsimony or the injunction against appealing to supernatural causes?¹³ And if it does not (here is the second question), is the concept of uniformitarianism a fruitful one? If “uniformitarianism” was suddenly erased from the geological lexicon, would it be necessary to reinvent it? Or would its elimination make no difference, or perhaps bring about a balance of benefit over harm? If there would be no reason to remind forgetful geologists that “the present is the key to the past,” and especially if there are good reasons to avoid such a reminder, then uniformitarianism is clearly unnecessary.

So is uniformitarianism redundant? That depends on what one means by “uniformitarianism.” If uniformitarianism is nothing but the claim that the laws of nature do not vary in space and time, then the case for redundancy is strong. All inductive inference relies on the postulate that the laws of nature are stable; as Gould (1965) observes, “Without assuming [the] spatial and temporal invariance [of natural law], we have no basis for extrapolating from the known to the unknown” (226). But notice that

¹³ If it *does*, then uniformitarianism is arguably unnecessary, since the term merely renames a principle or norm of reasoning better known by a different name.

this says nothing about geohistorical reasoning as such. It is rather a statement about the prerequisites of inductive inference in general. So uniformitarianism is redundant.

Alternatively, if uniformitarianism “comes down to the issue between naturalism and supernaturalism” (as Lyell liked to insinuate), then uniformitarianism “dissolves into a principle of simplicity that is not peculiar to geology but [instead] pervades all science and even daily life” (Goodman 1967, 95, 99). Here again uniformitarianism comes out as redundant, for as Goodman puts it, the modern geologist should feel no particular need to “[defend] himself [*sic*] for practicing what he fears may not quite be a science” (99).

But what if uniformitarianism cannot be reduced to a claim about the uniformity of nature, or to a gratuitous injunction against letting God figure in geological explanations? Is there an interpretation of uniformitarianism that is both non-redundant and interesting? There is, but it is a contested one. According to this interpretation, “the present is the key to the past” because the present earth provides a suitable model for any environment or process a geoscientist might want to reconstruct. This does not mean that the present earth contains a representative sample of all the environments and processes that have ever existed on the planet; everyone admits that it does not. Rather, the claim is that uniformitarian (or actualistic) models can be successfully applied to all sedimentary environments in the rock record, including those deposited under conditions that no longer exist on this planet. Uniformitarianism is “necessary,” it follows, because non-uniformitarian models are *un*-necessary. There are no circumstances in which a non-uniformitarian approach is warranted. In the past, many sedimentary structures were designated as non-uniformitarian (or “non-actualistic”) because the processes responsible for their formation were thought to have ceased operating (see, e.g., Cayeux 1941;

Pflüger and Grease 1996; Narbonne 1998). But in many or all of these cases, uniformitarian models have since been successfully applied.¹⁴ It follows that non-uniformitarian models are otiose.

The strongest statement of this position is found in Donaldson et al. (2002), who claim that “all studies involving Precambrian sedimentary rocks [the most common subjects of non-actualistic models] can be approached by means of comparisons to present day environments” (3). This claim has an air of authority to it; yet the authors clinch their argument by means of a semantic maneuver that ultimately evades the central issue. Donaldson et al. observe that the term “actual” (as in “actualism”) has two meanings. In vernacular English, it means *real* or *factual*; so something is “actual” if it really exists. Yet in many European languages, the word “actual” (as in the French *actualisme* or the German *aktualismus*) means *present-day* as opposed to *past*. The authors object to the custom of labeling “conditions not directly comparable to those of the present day” *non-actualistic*, following the customary European meaning (7). Instead, they propose to call any condition “actualistic” that can be explained “by valid physical and chemical laws.” Yet this confuses the issue at hand, which is whether the best way to approach “conditions not directly comparable to those of the present day” is “by means of comparisons to present-day environments.” Advocates of non-uniformitarian approaches suggest that the method of modern analogs is not the best way to understand all past environments, but the reason is not that they doubt that natural laws somehow fail to obtain in “non-actualistic” settings (Bottjer 1998). Their complaint is rather an epistemic

¹⁴ See, e.g., Eriksson et al. (1998); Donaldson et al. (2002); Virgili (2007).

one: perhaps “[the] modern world is not the best tool for explaining many times and events in the deep past” (Ward and Kirschvink 2015).¹⁵

What is the alternative to relying on the present world to guide interpretations of the past? In an article from 1995, paleontologists John Grotzinger and Andrew Knoll propose that “the key to the past” might sometimes be found within *the past itself*. Their topic was the anomalous reefs of the late Permian Period, which are composed of large volumes of aragonite and calcite marine cements, along with abundant microbial precipitates (microbially-induced mineral deposits). As Grotzinger and Knoll observe, the usual model for interpreting ancient reefs is uniformitarian (based on comparisons with modern reefs), and ascribes the accumulation of the “reef core” to “the upward propagation of an attached skeletal benthos” (578).¹⁶ It follows that to understand ancient reefs, researchers must first identify the organisms responsible for building the reef core, which in most cases are animals with calcareous skeletons like bryozoans and corals. Yet in the late Permian, all the usual reef-building suspects are absent. Further, it appears that the main frame is constructed, not by “the upward propagation of an attached skeletal benthos,” but rather “by marine cement growing directly on the seafloor”—a

¹⁵ I picked on Ward and Kirschvink in Section 2 for sliding from the claim that substantive uniformitarianism is false to the claim that methodological uniformitarianism is unreliable. Yet insofar as methodological uniformitarianism is understood as a procedure in which field observations are matched with observations from modern environments (e.g., Frodeman 2003), the failure of the modern earth to provide a representative sample of environments from earth’s past *is* methodologically relevant. Perhaps this is what Ward and Kirschvink have in mind.

¹⁶ Modern reefs are composed of corals and hard algae that overgrow one another, and in the process leave behind a “core” of limestone skeletons. This is the reef core.

phenomenon poorly represented in modern oceans. What then are we to make of these structures?

Grotzinger and Knoll's suggestion is to look backwards, to a period of earth's history known as the Proterozoic. There, before the major diversification of bilaterian animals, reef complexes were "dominated by massive marine cements" that accumulated directly on the seafloor. Some of these complexes were wholly inorganic whereas others were microbially mediated, but in any event, the massive buildup of carbonate minerals was facilitated by a range of conditions not typically found in modern oceans (including widespread deep water anoxia and calcium-enriched surface water). There is one exception, however: one period of time in which this unusual combination of conditions reoccurred. This was the period of time immediately following the end-Permian mass extinction, when oxygen levels plummeted and increased continental erosion flushed the oceans with bountiful calcium. In light of this, Grotzinger and Knoll suggest that "the key" to understanding the anomalous carbonate buildups of the Permian lies in the Precambrian, before the dawn of abundant, complex animals. The massive carbonate buildups of the Precambrian "demonstrate the tremendous reef-building potential carbonate precipitates when suitably oversaturated surface waters mix with upwelling deep water" (592). By contrast, "dependence on the Recent for sedimentological analogs has encouraged an overemphasis on the framework contributions of animals...and an underappreciation of the capacity of precipitated microbialites and seafloor cements to build reef-like structures."

The question of whether the present is the key to the past thus remains unsettled. Perhaps it is the case that the modern world provides the surest guide, in every instance,

to interpreting past environments and processes. But there seem to be cases in which the historical record itself seems to provide the key for accessing other compartments of the past (for another example, see Quiroz et al. 2019).¹⁷ The answer to the question of whether uniformitarianism is necessary turns on whether, and how often, this is in fact the case.

2. When is it appropriate to give (non-) uniformitarian explanations of past events? (Understanding why something happened)

The second question is related to, but different than the first. It concerns not the adequacy of uniformitarianism as a strategy for finding out what the past was like (a matter of descriptive understanding), but rather the issue of how we should explain past events (a matter of causal explanation). Many geoscientists think that, all things being equal, we should prefer uniformitarian explanations—explanations that do not postulate geological or biological processes operating at different rates and intensities than those operating today. Still, most recognize that uniformitarian explanations are not universally applicable, and this raises the question of when it is permissible to appeal to non-uniformitarian causes (e.g., bolide impacts and calamitous floods), including types of

¹⁷ Page (forthcoming) makes the same point. In her view, geologists ought to be “methodological actualists” (her term for methodological non-uniformitarians), since it is risky to use observed regularities to make inferences about the past when the stability of these regularities is unknown. While Page draws an unorthodox contrast between actualism and uniformitarianism (see fn. 9), her discussion highlights a methodological worry at the heart of this section: that sometimes the “key” to the past may be located in the historical record itself.

causes that are completely unknown in today's world (e.g., novel evolutionary mechanisms).¹⁸

That this question differs from the first can be gleaned from the fact that, after the general acceptance of the Alvarez hypothesis, few would deny any role for non-uniformitarian explanations in geohistory.¹⁹ As Erwin and Valentin (2013) observe, “Today, geologists recognize that the rates of geological processes have varied considerably through the history of Earth and that many processes have operated in the past that may not be readily studied today” (10). In other words, geologists now realize that substantive uniformitarianism is false, and that it is appropriate—under certain circumstances at least—to postulate processes operating at different rates and intensities than those acting today. Still, the question remains contested because geologists differ in their understanding of what constitutes appropriate circumstances for invoking non-uniformitarian explanations. So, for example, it remains an open question “[w]hether uniformitarian explanations can be applied to understanding events of the Ediacaran and

¹⁸ Some readers may wonder whether this is the same question as the first. Earlier I asked whether uniformitarian models can be used to interpret all sedimentary environments preserved in the rock record. Here I am asking whether uniformitarian causes can explain all events known from the rock record. The questions are doubtless similar, especially since the way uniformitarian models interpret the sedimentary record is by explaining features of the record (e.g., rock fabrics and sedimentary structures). Yet here the distinction between forms of understanding is useful; for there is a difference between explaining features of the sedimentary record—trace evidence—and explaining events known from the rock record—reconstructions based on trace evidence. And one way to capture this distinction is to say that the former adds to our descriptive understanding—our understanding of *what* happened, or what the past is like—whereas the latter adds to our understanding of *why* certain things happened.

¹⁹ The Alvarez hypothesis proposed that the pulse of extinctions at the end of the Cretaceous Period was caused by the impact of a large bolide, which brought a number of downstream kill mechanisms in its train (Alvarez 1997).

Cambrian [Periods]” (Erwin and Valentine 2013, 10). “Although [the issue] has not usually been framed in this way,” Erwin and Valentine continue, “we [believe] that debates over the nature of the geochemical evidence, the processes involved in the construction of Ediacaran and Cambrian ecological assemblages, and the processes of change in developmental evolution in early metazoans all involve differences of opinion as to whether a uniformitarian approach is appropriate.”

The issue, then, is not about whether the rates and intensities of geological processes have varied over time. Everyone agrees that they have, and virtually everyone agrees that the history of the earth is pockmarked with catastrophes, some of which owe to genuinely singular causes. The issue is rather one of standards (Love and Lugar 2013). Different scientists differ in their willingness to invoke “unique circumstances and contingent events” to explain unusual happenings in geohistory (Erwin 2015, 2). And the same goes for non-uniformitarian *types* of causes. Reverting to the case of the Cambrian explosion, some paleontologists are hesitant to posit that any unique evolutionary processes operated early in the history of multicellular animals. So Cooper and Fortey (1998) write that “[a] far more consistent view of evolution can be gained by [avoiding such postulates], and accepting that although the scarcity of ancestral forms can mean the earlier phase [of the Cambrian explosion] is often obscure, the latter [phase] is the result of standard microevolutionary processes over extended periods of time” (155). Cooper and Fortey prefer to postulate a long cryptic history of animals during which “standard” evolutionary processes operated, rather than invoke unique evolutionary mechanisms operative during the Early Cambrian. Erwin (2011), by contrast, thinks that non-uniformitarian explanations are preferable, whatever their costs in reduced “consistency.”

This is because Erwin thinks that the evolutionary process is non-uniformitarian in certain respects, and that the nature of the variation available to natural selection changes over time as gene regulatory networks become more complex.

It is thus clear why this second question remains unresolved. Geologists may agree that “the rates of geological processes have varied considerably through the history of Earth,” but this does not settle the question of when it is appropriate to invoke non-uniformitarian explanations. Needless to say, most everyone agrees that they should be invoked “when the evidence demands it” and not otherwise. But researchers differ in their judgments of what the evidence demands, and therefore in their judgments of when it is appropriate to invoke non-uniformitarian explanations. Usually this is because of other explanatory commitments they hold. Erwin, for example, thinks that the kind of variation available to natural selection changes over time, and for this reason is willing to sacrifice a “more consistent view of evolution” for what he regards as a more adequate (non-uniformitarian) view. Others disagree, and think that theoretical postulates of this type supply no good reason to abandon uniformitarian explanations of early animal evolution. Whoever is correct, it is clear that there is more going on here than an abstract disagreement about the adequacy of uniformitarian explanations as such, and for this reason, the adequacy of uniformitarian explanations remains contested.

3. Does the evolution of the earth-life system—including the changes associated with the Anthropocene—render uniformitarianism otiose? (Understanding complex earth systems)

The third question concerns the behavior of earth systems, and the viability of uniformitarianism as an assumption about earth system dynamics. Importantly, this

question is not centrally concerned with uniformitarianism as a means of understanding the distant past. Rather, it is concerned with whether knowledge of recent earth history provides a reliable means of interpreting the future behavior of earth systems. The worry is most strongly articulated in an article entitled “Limitations of Uniformitarianism in the Anthropocene” by Jasper Knight and Stephan Harrison (2014). In this article the authors ask whether “[the] changing dynamics of Earth systems in the Anthropocene, and the explicit involvement of human activity in Earth system processes and feedbacks” means that uniformitarianism is otiose—that “neither the ‘natural laws’ expounded by the Principle of Uniformitarianism nor reference to high-CO₂ periods in the past can be used as guides to [future] Earth system behaviour” (2). Because anthropogenic activities are “push[ing] the behaviour of many [earth] systems beyond the bounds of their natural variability,” observations of past or present earth system dynamics may fail to illuminate the future behavior of these systems. Or to put the worry another way: perhaps the earth is evolving away from a condition in which models of climate and environmental change can be safely built on uniformitarian assumptions.

As Victor Baker observes in a response to Knight and Harrison, “[the] use of analogs from Earth’s past to understand Earth’s future” is outside the traditional purview of uniformitarianism (Baker 2014, 78). Still there is a sense in which we can call assumptions about earth system dynamics “uniformitarian” if they conform to the expectation that the process dynamics and controls on earth systems are the same now as in the past. Or, to set our inferential sites in the other direction, the same now as in the future. As I noted in Section 3, an important reason geoscientists want to understand the past behavior of earth systems is to use this knowledge to predict the future behavior of

these systems, including their response to human-induced perturbation. But these inferences will only work if the process dynamics and controls on earth systems are the same now as in the past. If “Earth systems are now operating in ways that are substantially different to how they are believed to have operated in previous time periods,” projections from the past to the future will not work (Knight and Harrison 2014, 3). Likewise, if earth systems in the Anthropocene are becoming increasingly chaotic, “result[ing] in both complex system behavior and unpredictable outcomes as a result of [complex human-induced feedbacks],” then projections from the present to the future will not work either. In both scenarios, the assumption of uniform process dynamics and controls is to blame for the poor performance of models parameterized with data from the geological past (including the very recent past).

There is plenty to grumble about in Knight and Harrison’s article. The authors claim, for example, that “the Principle of Uniformitarianism...states that processes and products reflect constant conditions of external forcing,” and that “Uniformitarianism views systems as Newtonian, [meaning that] magnitude/frequency relationships follow a normal (Gaussian) distribution” (Knight and Harrison 2014, 3). Claims like these are idiosyncratic, to say the least, and I find no support for them in the classic enunciations of the “Principle of Uniformitarianism” (see Section 2). Still, the authors’ central concern seems to me a well founded one. If it is the case that earth systems are evolving towards increasingly chaotic and unpredictable dynamics, then models constructed on uniformitarian assumptions may indeed be in need of revision. In such a world, the present would no longer be the key to the past; nor would models based on the past behavior of earth systems be a reliable guide to the future.

4. *How useful are uniformitarian (or “actualistic”) studies of the geological record itself? (Understanding the geological record itself.)*

The final question concerns a kind of study designed to generate knowledge of the geological record itself, as opposed to this or that phenomenon in the past. Sometimes called “uniformitarian,” a more common term applied to these studies is “actualistic,” since they concern processes or factors currently in operation.²⁰ An important category of actualistic studies goes by the name *actuopaleontology*—literally “the paleontology of the present.” These concern how information enters the fossil record, as well as controls on the spatial and temporal resolution of fossil assemblages. Actuopaleontology encompasses many different kinds of studies, but a significant one is the “live-dead study”: a comparison of skeletal remains from a particular setting with live samples drawn from the same setting (Behrensmeyer et al. 2000). The idea is to get a handle on the spatial and temporal resolution preserved in fossil accumulations; that is, to probe the quality of the fossil record by comparing the taxonomic composition of so-called “death assemblages” with that of corresponding “life assemblages.”²¹ Many live-dead studies since the 1960s have taught paleontologists that “death assemblages tend to retain excellent environmental fidelity, reflecting variation in species composition even at spatial scales as fine as tens of meters” (Foote and Miller 2006, 14). Yet the same studies have also revealed that “the fidelity of death assemblages holds only for the readily preservable elements of the assemblage, and sometimes more strictly only for a single

²⁰ Recall that *actual* here refers to “present” or “current.” “Actualism” and “(methodological) uniformitarianism” are commonly treated as synonyms.

²¹ A death assemblage is an accumulation of partially-fossilized organic remains (called subfossils)—a kind of halfway house between a pile of dead animals and a *bona fide* fossil bed.

biologic group such as molluscs.” Fossil assemblages, then, are unlikely to be faithful copies of corresponding life assemblages, except for certain types of organisms, living in certain types of environmental settings.

It is at once clear why actualistic studies “[constitute] a major research direction [in] modern paleontology” (Kowalewski 1999, 452). For one thing, the fossil record is notoriously imperfect, and actualistic paleontology gives researchers a way of quantifying this imperfection for particular environments and taxonomic groups. (Other kinds of actualistic studies probe other aspects of the fossil record. Experiments with tumblers, for example, have explored how fossils decay under different environmental conditions. This teaches researchers about the conditions in which fossilization is most likely to take place, among other things.) Yet for all their utility in exploring aspects of fossilization, actualistic studies have their limitations as well. It is not clear, for example, what we are warranted to conclude from live-dead studies of contemporary environments about the quality of the fossil record in other geological periods. Perhaps ancient death assemblages preserved a similar percentage of taxonomic groups as Recent death assemblages; perhaps not. The point is that we do not know, or at least we do not know for a great many settings. (Sometimes the exceptional preservation of “whole communities” enables a more direct look at preservation under ordinary circumstances; but even here uncertainties remain.) More generally, paleontologists have questioned how often “[actualistic] observations [are] applicable...to the fossil record,” since conditions on the earth and associated taphonomic processes have changed considerably over time (Kowalewski 1999, 452).

There are (at least) two responses to this criticism, neither of which suffices to put the worry to rest. The first is given by Michał Kowalewski in the commentary cited above. Kowalewski admits that present-day observations may in many cases be inapplicable to the fossil record, yet he claims that “present-day observations...should not be discarded because they may be useful indirectly” (Kowalewski 1999, 452). He gives as his example the study of beach ridges, or “cheniers,” in the tidal flats of the Lower Colorado Delta. Now, the way cheniers are identified in unconsolidated rock is through the “shell luster features” of the shells composing the chenier. But shell luster is rarely preserved in consolidated rocks, and at any rate, cheniers tend to be smeared into oblivion before they can be preserved as distinguishable sedimentary structures. It follows that “a [straightforward] application of the actualistic study of cheniers is not appropriate” for the purpose of interpreting the sedimentary record. Still, the actualistic study of cheniers can be useful indirectly “because it can aid geologists in identifying those transgressive ravinements [low-relief surfaces formed by the landward migration of a shoreface] that were generated from the reworking of cheniers.” It turns out, for explicable taphonomic reasons, that the shells composing a chenier are typically derived from a single opportunistic species. “Thus, unless cheniers of different ages are thoroughly mixed during a transgression, the chenier-derived ravinements should retain the close-to-monospecific composition [of the original chenier].” So even though cheniers are not an important feature of the sedimentary record, the actualistic study of cheniers can help geologists identify the type and origin of particular ravinements—a key collateral benefit.

The second response to the worry that actualistic studies may not be applicable to the sedimentary record is to chronicle changes in taphonomic conditions over time, and to

evaluate their effects on the quality of the fossil record (Allison and Bottjer 2011).²² This promises to illuminate how far actualistic studies can be extended into the past; namely, just as far as the relevant taphonomic conditions can be shown to have obtained. For example, most existing Paleozoic and Mesozoic sediments (that is, sediments that formed between 541 and 65 million years ago) are lithified: they have been condensed and transformed into solid rock. Sediments that formed more recently, however, are increasingly unlithified; and unlithified sediments tend to preserve more biological diversity than lithified sediments. This means that live-dead studies in which the death assemblage is derived from unlithified sediments may paint a too-rosy picture of the fidelity of the fossil record, precisely because lithification itself tends to destroy biological information. In order for these studies to tell us about the record of diversity in deep time, they need to be corrected to account for this bias (see Bokulich 2018 for a general treatment of this strategy). And so it is with other taphonomic biases; hence the extent to which we trust actualistic studies to guide our interpretations of the fossil record hinges on our confidence that we have identified all the relevant biases and made the appropriate corrections.

Again, there should be no mystery as to why this question about uniformitarianism is contested. How useful one takes actualistic (uniformitarian) studies of the fossil record to be will depend, at least, on one's view of the collateral benefits of actualistic observations and on one's confidence in our ability to identify and correct for the biases in large datasets. Since neither the collateral benefits of actualistic studies nor

²² These large-scale trends in taphonomic conditions are known as “megabiases” (Allison and Bottjer 2011).

the ability of paleontologists to identify and correct for the relevant biases is obvious, there is much room here for disagreement.

5. Whither Uniformitarianism?

We have now reviewed four substantive questions raised by the application of “uniformitarianism” to different kinds of investigative goals (corresponding to different forms of understanding). Each question we found to be contested among practicing geoscientists; hence, it is little mystery that uniformitarianism continues to occupy an ambiguous place in geological discourse. The problem with “uniformitarianism” is not just its “kaleidoscopic usage in the literature” and whatever conceptual confusions this engenders (Romano 2015, 65). Rather, it is its entanglement with a number of substantive questions that remain unresolved—questions that concern standards and values as much as matters of fact. How useful are uniformitarian studies of the geological record really, given that the full scope of their application cannot be precisely delimited? And how hard should we push for a uniformitarian explanation before we explore non-uniformitarian alternatives? Questions like these do not have straightforward answers, and it is therefore unsurprising that they elicit different responses from different scientists. It is to be expected, then, that uniformitarianism will continue to excite geological passions for the foreseeable future.

I imagine that this conclusion will strike certain readers as unsatisfying. Sure, scientists might disagree about the all-sufficiency of uniformitarian models or the usefulness of actualistic studies of fossilization, but isn’t the point of a philosophical intervention to see past these local dust-ups to the heart of the matter? In short, shouldn’t

a philosophical study of uniformitarianism tell us whether the present really *is* the key to the past? I don't think so. Sometimes the role of philosophy is to add nuance to discussions of a contested subject, and the debate over "uniformitarianism" is a debate that badly wants for nuance. As I have argued in this chapter, the question of the adequacy and applicability of uniformitarianism is a complex one, since the term "uniformitarian[ism]" attaches to different kinds of things: reasoning strategies (and associated models), explanations, assumptions about earth system dynamics, and studies. To probe the concept of "uniformitarianism," then, is to probe the value of these diverse things in equally diverse investigative contexts. It might be the case that uniformitarian models can be applied to every environment in the sedimentary record, *and also* that uniformitarian explanations have definite limits, or that actualistic studies of the fossil record can be used to illuminate only certain stretches of geological time. If this is true, then uniformitarianism stands both acquitted and condemned: vital to geological methodology and yet limited in deep and meaningful ways.

All this suggests that there is no simple answer to the question, "Is the present the key to the past?" On the one hand (to recall a worry from Section 4), there seem to be situations in which the key to the past is found within the historical record itself. So, for example, maybe the key to understanding late Permian reef complexes lies within the Precambrian, prior to the rise of animal-dominated ecosystems. Only in the Precambrian were the shallow oceans sufficiently devoid of animals to "model" the conditions found after the end-Permian mass extinction, the greatest biological calamity of the past half-billion years. Maybe it is even the case that, in the absence of the Precambrian sedimentary record, the stromatolitic interpretation of late Permian reefs would have been

permanently foreclosed (although this strikes me as unlikely). Still, having said all this, it is not the case that the key to the *Precambrian* world was discovered in rocks of Precambrian age. Rather, it was discovered in the present: in studies of the stromatolites of Shark Bay, which clinched the case for the biological origin of stromatolites (Knoll 2003). This suggests that the present really does supply a key to the late Permian—or at least a key to that compartment of the past that contains a key to the late Permian. Similar cases are most likely similarly convoluted, excepting those cases in which no keys exist for the relevant door. (Might the Ediacara biota be trapped behind such a door?)

Thus my title: “The Present is the Key to the Past, Except When it Isn’t (And When Then it Kind of Is).” This wouldn’t look great on a bumper sticker, and I don’t expect to see it in a geology textbook any time soon. But probably it is the best we can do, at least for the time being. To say anything stronger is to trade rigor for specious clarity, and to pretend that the problem of understanding geohistory is simpler than it actually is.

Chapter 5:

Biased, Spasmodic and Ridiculously Incomplete: How Sequence Stratigraphy Helped to Unlock the Potential of the Fossil Record ¹

1. Introduction

In 1995, Stephen Jay Gould issued the last of what his colleagues jokingly described as his “state of paleontology” addresses. The occasion was the twentieth anniversary of the founding of *Paleobiology*, the journal that had served as the focus and principal mouthpiece of the “Paleobiological Revolution” (Sepkoski 2012).² Gould’s title

¹ This chapter includes an Appendix, which provides a glossary of important stratigraphic terminology. It begins on page 256.

² The Paleobiological Revolution was a campaign to upgrade the disciplinary standing of paleontology, and saw the emergence of paleobiology as a distinct area of study “centered around the quantitative analysis and interpretation of the history of life” (Sepkoski 2013, 402). It is usually dated from about 1970 to 1985, and associated with

was “A Task for Paleobiology at the Threshold of Majority [legal adulthood].” It was designed to contrast with his earlier article, “The Promise of Paleobiology as a Nomothetic, Evolutionary Discipline,” written to commemorate *Paleobiology*’s first five volumes (Gould 1980). In this earlier “address,” Gould exhorted his colleagues to slip the yoke of stratigraphic geology—a yoke that saw fossils relegated to “an empirical tool” for “stratigraphic ordering and environmental reconstruction” (Gould 1980, 98). In addition, he urged them to abandon their “passive” stance before evolutionary theory and to assume the role of active and creative contributors. Gould himself had modeled this role when, in 1972, he debuted the theory of punctuated equilibria with co-author Niles Eldredge (Eldredge and Gould 1972). Yet in 1980, contributions like this remained scarce, and Gould looked forward to a time when paleobiology could boast a large brood of theoretical insights, testifying to its unique role as the “guardian of the record for vast times and effects.”

Fast forward to 1995, and the “promise of paleobiology” had largely been fulfilled. In thrall to no one, the new discipline had emerged as a valued (if frequently controversial) contributor to evolutionary theory (Ruse and Sepkoski 2009; Sepkoski 2012). And the journal *Paleobiology*, little more than a fledgling in 1980, had become an institution. In Gould’s estimate: “*Paleobiology* has won attention and respect because it has been the active centerpiece for a scientific movement that will largely define paleontology in our time” (Gould 1995, 3). What remained was for paleobiologists to complete their Revolution by solidifying their status as valued, and indeed invaluable,

names like Gould, David Raup, J. John (“Jack”) Sepkoski and Thomas Schopf (see Sepkoski and Ruse 2009; Sepkoski 2012).

contributors to evolutionary theory. This involved two tasks. First, paleobiologists were to characterize large-scale patterns in the history of life; then, they were to explain these patterns using a body of home-spun evolutionary theory. Gould was careful to note that theory alone “does not define the task of paleobiology,” since “narrative patterns of life’s long-term history are as important as theories invoked to explain them” (Gould 1995, 7). Still, “the twin themes of macroevolutionary theory and [large-scale] pattern work together to define the task of paleobiology for the evolutionary sciences” (12, emphasis added). And since the evolutionary sciences are paleobiology’s rightful home, these themes come close to exhausting the task of paleobiology as a whole.

Like many of the things Gould wrote, this article was not a little self-serving. Few would have failed to notice that his “task for paleobiology” awarded a central place to just those theoretical activities in which Gould was already engaged (Dresow 2019). Moreover, his “task” was singularly, and characteristically, focused on evolution (Bambach 2008). “[The] development of macroevolutionary theory and its application to understanding the pattern of life’s history. That is our task,” Gould declared (Gould 1995, 3). The vision is striking, if only because the scope and diversity of paleobiological research had exploded since the early days of *Paleobiology*. During that time, paleobiologists had studied everything from the chemical composition of fossils to microbial controls on geochemical cycling in the Neoproterozoic. So broad was its remit that, by 2000, Douglas Erwin and Scott Wing could write that “[the] diversity of paleobiological research seems almost to defy unification” (Erwin and Wing 2000, ii).

A particularly promising strand of research involved the application of new models of sedimentary basin filling to the problem of understanding the stratigraphic

distribution of fossils. In fact, the very issue of *Paleobiology* that contained Gould's "Task" also contained a paper by Steven Holland that brought this approach into the domain of numerical modeling, with celebrated results (Holland 1995a). Armed with these models, paleobiologists turned increasingly to questions that "require the collection of fossils in a field context and directly incorporate field data into their solutions" (Droser 1995, 507). These questions were biological in nature, and concerned things like the relative timing of physical and biological events, and the interpretation of biological patterns documented at the scale of outcrops to sedimentary basins (Patzkowsky and Holland 2012). Yet they differed from the questions that were focal during the Paleobiological Revolution by concerning regional, as opposed to global, patterns, and by focusing as much on ecological as on evolutionary considerations. What's more, they incorporated a basically novel approach to stratigraphic complexity; one grounded in the deployment of high-resolution frameworks for analyzing the incompleteness of the fossil record, and for parsing controls on the distribution and abundance of fossil taxa. But more on this in a moment.

Gould's cramped view of paleobiology has tended to be reflected in recent historical scholarship on the field. The reason is that this literature has focused almost entirely on the Paleobiological Revolution, and in particular, on the period from about 1970 to 1985.³ Apropos of this focus, two themes have come to play coordinating roles in the historiography of paleobiology. The first is that paleobiology is a *biological* (read: *evolutionary*) science, which is tasked above all with contributing to *evolutionary theory*

³ As I observed in Chapter 1, a similar restriction of focus has shaped philosophical reflections on paleontology (usually understood narrowly to mean paleobiology).

(Bambach 2009; Grantham 2009; Ruse 2009; Valentine 2009; Baron 2011; Turner 2011; Sepkoski 2009, 2012, 2019). So Derek Turner writes that the Paleobiological Revolution was undergirded by a belief that “Paleontology has more to contribute to [evolutionary] biology than to geology” (Turner 2011, 7). He continues:

The study of fossils has long played an important role in geology, in part because understanding fossils is helpful for identifying and dating types of rocks. [Yet the] 1970s revolutionaries sought to move their field closer and closer to biology, and one way in which they tried to do that was to show that they had something to say about *evolutionary theory*...Some started using the term “paleobiology” to describe their work. This was a way of signaling that the game had changed; paleontologists were now contributing to *evolutionary theory*. (Turner 2011, 7, emphases added)

David Sepkoski has a similar take:

The dilemma faced by paleontologists in establishing paleontology as a legitimate *evolutionary discipline*...involved both asserting the theoretical value of the fossil record and repositioning paleontology within the larger matrix of *evolutionary biology*...The new model paleontologist was trained in biology as well as geology, was adept at quantitative analysis, was prepared to employ general theoretical models *to explain how evolution worked*, and might be more comfortable seated at a computer than at a fossil preparation table. (Sepkoski 2012, 2–3, emphases added)

To establish paleontology as an *evolutionary discipline*, paleontologists needed to reposition their field within the matrix of *evolutionary biology* and, ultimately, demonstrate that paleontology has distinctive contributions to make to *evolutionary theory*. Not for nothing did Sepkoski subtitle his 2012 monograph on the Paleobiological Revolution, *The Growth of Paleobiology as an Evolutionary Discipline*.

The second theme is related to Sepkoski’s observation that the new paleontologist

is wont to prefer the computer bench to the preparation table (and, we might add, the museum stacks to the outcrop). It is that paleobiology is a science *centered on the analysis of large amounts of data*—it is not an activity concerned with the classification and interpretation of particular specimens. As Sepkoski (2013) observes, data-centric practices have a long pedigree in paleontology. From its first stirrings as a discipline, paleontologists have employed technologies for managing and analyzing copious data, from lavishly illustrated atlases to tabular compilations of the fossil record supplemented with interpretive diagrams (Rudwick 1972; Sepkoski 2013, 2017; Tamborini 2019). Yet something important changed during the 1970s and 1980s. With the advent of digital computers and computerized data, “large-scale quantitative analyses of patterns in the history of life” become the stock-in-trade of the new evolutionary paleobiologists (Sepkoski 2012, 2). The history of data practices in paleontology, it follows, involves significant ruptures imposed upon no less significant continuities.

Because the historiography of paleobiology has centered on two themes—paleobiology as an *evolutionary* and *data-centric science*, respectively—inquiry has focused on two topics: (1) the drama of paleontology’s relationship with evolutionary theory and (2) the history of practices used to characterize large-scale patterns in life’s history. Yet this leaves sizable gaps in our understanding, most notably concerning paleobiology’s status as a *geoscience*. It was no aim of the 1970s revolutionaries to uproot paleobiology entirely from the geosciences, after all. Even Gould, the most biologically-oriented of the Young Turk paleobiologists, affirmed “the absolute necessity of comprehensive geological training for success in paleontology”—and in a paper touting paleobiology as an evolutionary discipline, no less (Gould 1980, 98). “[With] all

biology and no geology, paleontology is empty; but with geology alone it is blind,” Gould claimed. It is “blind” without biology because paleontology’s most important questions come from evolutionary theory (or so Gould tended to suppose). Yet it is “empty” without geology because in its working methods and visual language, there is much in paleontology-cum-paleobiology that is distinctively geological.

This chapter begins to fill the gap in our understanding of paleobiology as a geoscience. It does this by tracing the emergence of a new approach to stratigraphic complexity: first in geology, and then, following its creative appropriation, in paleobiology. The approach is associated with a set of models that together revolutionized stratigraphic geology during the 1970s and 1980s. These included the celebrated sequence stratigraphic models of Peter Vail and others, which show how the processes of sedimentary accumulation impart a complex structure to the stratigraphic record. Transposed into paleobiology, they gave researchers a powerful way of studying the incompleteness of the fossil record, and of removing biases imposed by the structure of the rock record itself. In addition, they helped to reconfigure the cultural landscape of paleobiology, giving a new impetus to fieldwork and eroding the barrier that Gould and others had erected between the “old” and the “new” paleontology. In the words of Mary Droser, a field-based paleobiologist writing in 1995:

Paleobiology is broadening as it matures and finding answers in the field sciences. With this, the lines that separate divisions of paleontology become even more vague. Those who consider themselves paleobiologists or [field-based] paleontologists may [even] find themselves with similar objectives. (Droser 1995, 508)

The remainder of this chapter is organized in six sections. In Section 2, I survey the strained relationship between paleontology and stratigraphy in the lead-up to the Paleobiological Revolution. This section also reviews the strategies that paleobiologists developed to cope with the incompleteness of the fossil record, and explains why these failed to provide much guidance for the interpretation of biological patterns at local and regional scales. Section 3 provides a whirlwind tour of some important ideas and developments in twentieth century stratigraphy. These form a springboard to Sections 4 and 5, which explore the development of event and sequence stratigraphy, respectively. In Section 6, I consider how these developments were appropriated by paleobiologists, focusing on the influential simulation models of the “stratigraphic paleobiologist” Steven Holland (1995a, 1995b). Then, in Section 7, I consider how the advent of stratigraphic paleobiology shaped the development of the field, not least by shortening the cultural distance that had opened up between the old (field-based or stratigraphic) paleontology and the new paleobiology. I close, in Section 8, with a brief reflection on how stratigraphic paleobiology has begun to change the conversation surrounding incompleteness and bias in the fossil record.⁴

⁴ A note for philosophical readers. This chapter is a historical study, and as such it does not engage philosophical themes directly. Still, the chapter points towards several issues of philosophical interest. The first concerns ideas about the rock record. To date, philosophers of science have been remarkably uninterested in the rock record as such, saying little more than that it is stratified (i.e., layered). But the rock record is not just stratified; it is also intricately structured, and this structure both complicates and facilitates attempts to reconstruct geohistory (at least once the structure has been adequately understood). Recently, Alisa Bokulich has examined how paleobiologists use models to “correct” data in large datasets (Bokulich 2018). We need parallel investigations of how paleobiologists and other geoscientists use models (like those discussed in this chapter) to guide the interpretation of field data and to inform sampling strategies. This is a distinct form of practice from that discussed in Bokulich (2018), in

2. The Paleobiological Revolution and Beyond

The Paleobiological Revolution was premised, in no small part, on a desire to liberate paleontology from the clutches of stratigraphic geology (Baron 2011; Sepkoski 2012). This was necessary, it was argued, in order that paleontologists might cultivate a genuinely biological attitude towards their subject matter: the fossilized remains of past organisms. Fossils have a variety of uses in geohistorical research, not the least of which is as tools for subdividing the rock column and correlating distantly separated stratigraphic sections. Yet nothing in these practices requires the investigator to regard fossils as the remains of once-living organisms as opposed to neutral markers of stratigraphic position. As such, stratigraphic paleontology, which provided the main employment for invertebrate paleontologists during the first half of the 20th century, failed to develop a pronounced biological orientation.⁵ Its questions remained geological

part because it targets a different scale of investigation, and in part because it has different aims.

A second theme concerns notions of incompleteness and bias. As I note in this chapter, the idea that the fossil record is incomplete and biased has a long pedigree in paleontology. It is also a prominent feature of philosophical analyses of the historical sciences, which have tended to highlight evidential degradation as a major challenge facing historical scientists (see Chapter 1, in particular, fn. 8). But stratigraphic paleobiology puts pressure on this simplistic characterization. Sure, the fossil record is “incomplete” and “biased” (e.g., skewed towards hard parts), but no less important, it is *structured*—and this structure can be investigated to inform data collection and to aid in model specification. What this means is that philosophers should not assume that incompleteness (driven by evidential degradation) is an appropriate characterization of paleontologists’ epistemic situation. In addition, they should take a second look at whether there is anything unusual about the “incompleteness” and “bias” of the fossil record when compared to other scientific datasets.

⁵ This was the case in the Anglophone world, at any rate. In the German-speaking world, the relationship between stratigraphy and paleontology seems to have been less strained (Tamborini, personal communication). It is possible that this owed to the influence of figures like Heinrich Bronn (1800–1862) and Karl Alfred von Zittel (1802–1871), whose

and utilitarian, and its practitioners remained narrow taxonomic specialists as opposed to question-driven biological researchers. J. Brookes Knight, an invertebrate paleontologist, put the point tartly in his 1947 presidential address to the Paleontological Society:

[What] we today call a paleontologist, particularly that jellylike variety without a backbone, incapable of standing erect on his own two feet, the invertebrate paleontologist, is not a paleontologist at all. He is a geologist, a stratigraphical or “soft-rock” geologist. He has considerable familiarity with invertebrate fossils, to be sure, but he is a geologist nevertheless. (Knight 1947, 284)

The problem had not abated by 1968, when Martin Rudwick (then working as a paleontologist) complained that paleontology had been “stunted throughout its existence by its subservience to the needs of stratigraphy”:

This [subservience] has hindered the mainstream of paleontological work from developing any genuinely biological attitude. The situation has certainly improved within the last decade, but even today what is so often missing is any imaginative awareness of fossils as the remains of organisms that were once alive. (Rudwick 1968, 35)

Even in 1980, the situation was dire enough to cause Gould to write the following:

Invertebrate paleontology has cast its institutional allegiance with geology—more by historical accident than by current logic. When it operates as a geological discipline, paleontology has tended to be an empirical tool for stratigraphic ordering and environmental reconstruction. As a service industry [of academic and economic geology], its practitioners have been schooled as minutely detailed, but restricted experts in the niceties of taxonomy for particular groups in particular times. (Gould 1980, 98)

“data-driven” approaches fostered a biological integration of paleontological and stratigraphic data (see Sepkoski 2017; Tamborini 2017; Sepkoski and Tamborini 2018).

All this points to a consensus among would-be paleobiologists: (1) that the science of paleontology had been unjustly subordinated to the project of stratigraphy; (2) that progress in paleontology required the cultivation of a genuinely biological attitude; and (3) that because the subordination of paleontology to stratigraphy prevented the cultivation of such an attitude, the future of paleontology hinged on its liberation from stratigraphic geology, as well as the reassertion of its status as an autonomous biological science.

But how was all this to be achieved? Gould, at least, had a plan. If paleontology was to slip its fetters and step forth as an autonomous biological discipline, it needed to cultivate a new attitude toward its data, on the one hand, and evolutionary theory, on the other (Gould and Eldredge 1977; Gould 1980). This involved breaking a loop of reinforcement that saw paleontology relegated to the status of a mere satellite of evolutionary theory—a source of data about life’s empirical pattern, but *not* a contributor of new ideas. For Gould, it all began with the fossil record. Since the middle of the 19th century, it had been a commonplace that the rock record (including the record of emplaced fossils) is woefully incomplete. Charles Darwin referred to it as “a history of the world imperfectly kept,” and proceeded to deny that we have any right “to expect to find in our geological formations, an infinite number of those fine transitional forms, which on my theory assuredly have connected all the past and present species” (Darwin 1859, 310, 301). Later paleontologists mostly agreed with his assessment (Eldredge and Gould 1972; Sepkoski 2012). It is not the case that here and there in the pile of formations, a page has been torn from the record of earth’s history. To the contrary, the record is missing most of its pages, and of those that remain, the majority are “torn” or

“blotted,” or else covered in confused writing like a palimpsest. It was this dim view of the fossil record that Gould and others sought to reform “through a deliberate manipulation of Darwin’s book metaphor” (Sepkoski 2012, 3). If the fossil record was “an imperfect text,” the strategy of paleobiologists would be to “reread” that text in a way that enabled them to frame valid generalizations about large-scale patterns and processes in the history of life.

Sepkoski (2012) identifies three strategies that paleobiologists developed to cope with the incompleteness of the fossil record. The first he calls “literal rereading,” and consists in a disposition to read certain aspects of the fossil record, at least, at face value. So Eldredge and Gould claim that “[m]any breaks in the fossil record are real; they express the way in which evolution occurs, not the fragments of an imperfect record” (Eldredge and Gould 1972, 96, emphasis added). The second strategy is its polar opposite. Inspired by the success of abstract modeling practices in ecology, “idealized rereading” was designed to circumvent the messiness of the fossil record by simulating the history of life *in silico*. In these simulations, “the physical particulars of the fossil record [are] all but ignored, and the history of life...is modeled as a series of homogeneous data points...using very simple parameters” (Sepkoski 2012, 3–4). The results of these simulations could then be compared to the fossil record to make certain inferences about the history of life. Finally, there is the approach that Sepkoski calls “generalized rereading,” which involves the assembly of data in large electronic databases for the purpose of framing “statistical generalizations...about patterns in life’s history” (Sepkoski 2012, 4). Crucially, this approach is “marked by meticulous collection of data on a monumental scale and by an interplay between mathematical modeling and

rigorous, insightful data analysis” (Foote 1999, 326). It is not just an attempt to smooth out a patchy fossil record with copious data (see also Bokulich 2018).

As Sepkoski observes, generalized rereading emerged in the 1970s and “ultimately became the dominant methodology in analytical paleobiology during the 1980s” (Sepkoski 2012, 4). Yet generalized rereading was most easily applied to problems at the largest spatial and temporal scales (e.g., global studies of marine diversity through time). For studies of individual basins, not to mention bed-by-bed studies of morphology or taxonomic change, other strategies were more suitable, like literal rereading.⁶ But literal rereading had well-known limitations, as even Gould was fast to admit (Gould 1969). Compounding the problem, growing knowledge of the processes that structure the rock record suggested that the pattern of fossil occurrences is strongly shaped by the structure of the rock record itself (e.g., Kidwell 1991; MacLeod 1991; Brett 1995). Some strategy was therefore needed for taming these biases—for extracting biological signals from the scrambling and distorting effects of stratigraphic overprint. This researchers would eventually find in sequence stratigraphy, but only after a new generation of paleobiologists had traversed the cultural gap that had opened between paleobiology and its erstwhile oppressor.

The next three sections explore the roots of this new approach to stratigraphic complexity by examining those developments in stratigraphy that made possible a fruitful reconciliation with paleobiology after the Paleobiological Revolution. In Section 3, I

⁶ Idealized rereading was not an option, since it is not an approach to “reading” the paleontological record at all, but rather an alternative to reading it.

explore the background to the revolutionary developments in stratigraphy during the 1970s and 1980s. Then, in Sections 4 and 5, I unpack the content of these developments.

3. Stratigraphy Before 1970: From the Layer Cake to the Crazy-Quilt

Stratigraphy is the study of layered rocks, and layered rocks are the archives of geohistory: “the sum of a thousand narratives in stone-stacked order” (Fortey 1997, 8). A major goal of geology is to piece together these narratives from scattered evidence, and to fit them into appropriate spatial and temporal frameworks (the tasks of reconstruction and correlation, respectively). Stratigraphy is central to both these tasks, and for this reason has been called “the heart of geology” (Weller 1947, 570), “the basic activity of geology” (Greene 2009, 171) and “the key to understand[ing] the Earth, its...structure and past life” (Doyle and Bennett 1998, 1). Indeed, since the beginning of the nineteenth century, the main activity of geologists “has been to name and measure every stratum of every sequence on earth, to detail its component minerals, and to reconstruct the story of its formation, its existence, and in many cases its deformation and destruction” (Greene 2009, 171). This activity is basically stratigraphic. Elaborate reconstructions of geohistory are built upon the frameworks supplied by stratigraphic research; so in this sense it is no exaggeration to say that “stratigraphy...underpins the whole basis of Earth history” (Benton and Harper 2009, 25).

Central to stratigraphy throughout its long history has been paleontology, since fossils provide the best means of correlating rocks over hundreds, or even thousands, of kilometers. Enthusiasm for this practice blossomed during the early decades of the nineteenth century due to the recognition that many systems established on the basis of

fossil evidence could also be recognized abroad, even on different continents (see, e.g., Rudwick 1985; Rupke 1998). Guided by fossils, geologists were able to see past “the bewildering variety of local formations and the confusing effects of local tectonic disturbances,” to articulate a consistent outline of geohistory for the largest divisions of geological time (Rudwick 1972, 199). Indeed, with the refinement of stratigraphic methods during the middle of the century, hopes were high that the rock record might be decomposed into a predictable succession of “zones” with global, or near-global, applicability.⁷ The rock record might then be pictured as a layer cake, with each layer representing a unique interval of time as well as a group of strata corresponding to that interval (Brett and Baird 1997).

Although many nineteenth century geologists held views that later generations would malign as “layer cake stratigraphy,” all were aware that the stratigraphic record is complex, and that this complexity has an important spatial dimension. In the present day, environmental conditions are highly variable from place to place. Even at a single place, a range of environments are liable to be found in close proximity, each with its own complement of biological inhabitants. Conditions in the past were probably similar, at least in the sense that many environments are likely to have existed side by side, forming a mosaic of environmental conditions. It is to be expected, then, that this mosaic will be reflected in the rocks as a mosaic of lithological and paleontological characteristics. As the British geologist Henry De la Beche wrote in 1839, it is most unlikely “that detrital

⁷ The term “zone” was used in a number of ways in the nineteenth century (Hancock 1977). Most influentially, it referred to a stratigraphic interval characterized by a particular fossil assemblage, although zones were typically named for a single fossil taxon. (See the Appendix to Chapter 5, pp. 256–259, for a complete glossary.)

matter has been strewed in exactly the same manner, enveloping exactly the same organic remains, over all parts of the world, where deposits were taking place at the same time” (39). His point was directed against those who assumed a contrary picture in their practice: in particular, those who assumed that the rock record could be analyzed as a stack of mostly homogeneous strata, each with a distinctive set of fossils that fixed the location of that stratum uniquely in the pile.

The term “facies” was introduced in 1838, and signaled an increased recognition that the characteristics of a rock unit can vary considerably from place to place (Teichert 1958; Hancock 1977).⁸ Yet the notion that the rock record resembles a layer cake—one with dappled layers, perhaps, but a layer cake nonetheless—persisted into the twentieth century (Brown 2013). Especially in North America, stratigraphers continued to describe strata as laterally continuous sheets of mostly homogeneous rock, with the presumption that these corresponded to unique intervals of time (Brett et al. 2007). These stratigraphers were aware that rock type often shifts as you trace a formation laterally (the phenomenon of lateral facies change). Moreover, they did not believe that all rock units of consistent lithology were necessarily “isochronous,” or equivalent in age over their entire extent. Still, stratigraphers during this period remained largely absorbed in the tasks of naming and mapping broad packages of strata over extensive geographical areas (Miall 2010). And for these tasks, layer cake views served passably well. Sure, the stratigraphic record may not form a perfect layer cake, but it approximates one well enough that geologists can get on with the work of delimiting major packages of strata,

⁸ “Facies” is a Latin word meaning “face” or “external appearance.” In geological usage, it means a sedimentary deposit characterized by a set of features that permit its environment of origin to be inferred (e.g., coastal plain, reef front, deep water).

and correlating them between localities using various kinds of evidence. All that is required is that laterally continuous strata units exhibit some degree of isochrony: in other words, that units traceable across country not differ markedly in age from place to place. (Rock units that differ in age from place to place are known as “diachronous.”)

The story of stratigraphy in the middle of the twentieth century is a story of the erosion of confidence in lateral continuity. Indeed, by the 1960s, the influential stratigrapher Alan B. Shaw could speak with authority of “the universality of diachronism”—that is, of the notion that all sedimentary rocks deposited in stratigraphically important environments are diachronous (Shaw 1964, x). According to this view, similar-looking and ostensibly continuous strata should *not* be regarded as roughly equivalent in age. Instead, they should be regarded as merely analogous facies, and therefore as probably diachronous. (As Carlton Brett summarizes this view, “if a rock unit looks the same in two different places it must be of different ages” (2000, 496).) Likewise, faunal occurrences should be interpreted as diachronous unless members of the fauna belonged to widespread and short-lived taxa, termed “index fossils” for their usefulness in telling time. By the 1960s, the majority of stratigraphic complexity was analyzed in facies terms, and little attention was given to the project of tracing laterally continuous strata over extensive geographical areas (Brett et al. 2007; Miall 2015). As a consequence, paleontological methods were largely expelled from the mainstream of stratigraphic research, although they continued to find employment in the rapidly expanding field of energy exploration (see Newell 1962).

The 1960s are sometimes described as a “revolution” in lithostratigraphy. Out was the practice of tracing broadly continuous strata over large areas, mostly on the strength

of fossil evidence. Out too was the practice of representing these strata as “uniform blankets bounded by sharp vertical lines [on] the correlation table”—echoes of the old layer cake view (Miall 2000, 4). In their place were substituted facies analysis and diagrams showing “crazy-quilt” patchworks of lithostratigraphic blobs (see Figure 11).⁹ Developments in stratigraphic nomenclature consolidated this trend, calling for a separation of units based on rocks (the subject matter of lithostratigraphy), fossils (biostratigraphy) and time (chronostratigraphy). By all accounts, the nomenclatural innovations were successful, and “put an end to the days when a single term might represent a rock unit, a time unit, or some hopelessly confused combination [of the two]” (Holland 1999, 409). Still, a growing emphasis on “pure lithostratigraphy” added tremendously to the complexity of the geological literature. In addition, it “sidetracked the field from the central [issue] of reconstructing Earth’s history,” because it discouraged inquiry into the complex interrelationships of rocks, fossils and time (Holland 1999, 409). Or, to hear Carlton Brett tell it, “[in] their adherence to a stratigraphic code stating that rock units must be kept strictly separate from time units, lithostratigraphers almost lost the most critical of all notions: the perspective of the temporal scope of rock layers” (Brett 2000, 496).¹⁰

⁹ Facies analysis aims to “identify different environments in ancient rocks, and also to understand the range of processes that can operate within these environments” (Walker 1984, 5).

¹⁰ The perception that traditional stratigraphy had grown moribund in the 1960s and 1970s was a widespread one. Bhattacharya and Abreu (2016), for example, describe the early 1970s as the “end of stratigraphy,” since at the time undergraduate education in geology “focused almost exclusively on process sedimentology, facies analysis, and petrographic description, [as opposed to] stratigraphy [*per se*]” (7). Comments like this illustrate how far the revolution in lithostratigraphy carried the field away from its traditional preoccupation with delineating and correlating chronostratigraphic packages.

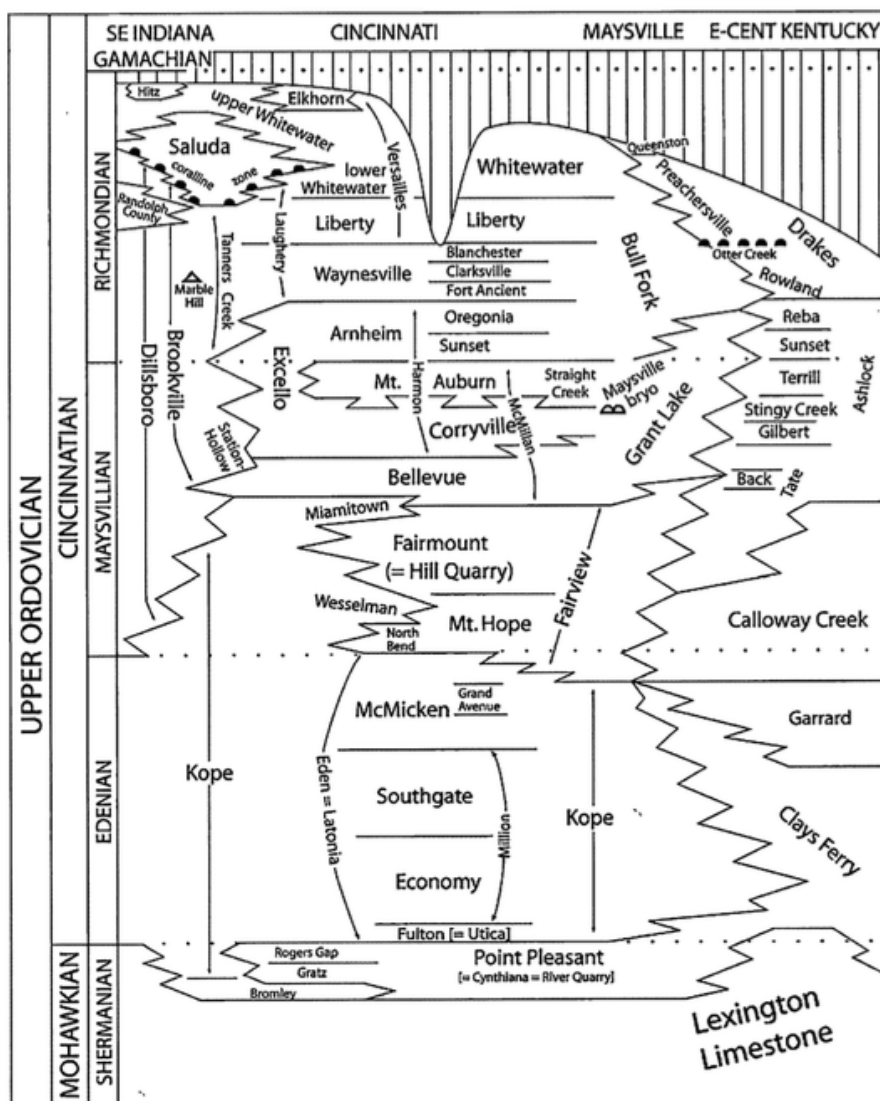


Figure 11 A diagram of the Cincinnatian strata of Indiana, Ohio and Kentucky, showing lateral relationships between “lithostratigraphic blobs” (named facies units). (From Cuffey 1998)

The eclipse of time in stratigraphy was a temporary one, and in the next two sections, I will review several ways that mainstream stratigraphers regained their interest in the temporal scope of rock layers. Before coming to this, however, it is worth pausing on the question of why the alienation of litho- and biostratigraphy resulted in a curiously

ahistorical stratigraphy. For all of the nineteenth and much of the twentieth century, biostratigraphy provided the primary means of determining the temporal relationships of rock bodies (a task that would come to be known as “chronostratigraphy”). Although radioisotopic methods were known from the early twentieth century, most rocks cannot be dated using isotopes, and other methods (like magnetostratigraphy calibrated to an absolute time scale) lacked the resolving power of biostratigraphy. When lithostratigraphy was “revolutionized” in the 1960s, then, biostratigraphy and chronostratigraphy were effectively the same project (Shaw 1963).¹¹ It follows that when lithostratigraphy expelled paleontological methods, the mainstream of stratigraphic research lost its best means of establishing time scales for the smaller divisions of geological time. It is this alienation that is in focus for complaints that stratigraphy got “sidetracked” in the 1960s and 1970s, and that it “lost the most critical of all notions: the perspective of the temporal scope of rock layers.”

There is an irony in all this: that while invertebrate paleontologists were bemoaning their subordination to stratigraphic geology, paleontological methods were in the process of being expelled from the mainstream of stratigraphic research (Newell 1962). Still, it would not be long before geologists were reminded of the temporal scope of rock layers, as well as Darwin’s great bugbear: the imperfection of the rock record. And perhaps unsurprisingly, it would be a paleontologist who would do the reminding.

¹¹ This is not to say that biostratigraphic (rock) units were ever chronostratigraphic (time) units—although in practice they were frequently treated as such (Hedberg 1965). It is just to say that, in the 1960s and 1970s, fossils provided the key line of evidence for chronostratigraphic dating.

4. More Gap Than Record

“*There is something damn funny about the stratigraphical record.*” So wrote the paleoecologist Derek Ager in a volume whose slim dimensions belie its enormous importance in the history of stratigraphy (Ager 1973, 1).¹² The volume is a self-proclaimed “ideas book,” which, given the reputation of most stratigraphers as narrow-minded empiricists, might have been expected to produce a small impact on the field (Ager 1993a, ix). Yet as the stratigrapher Andrew Miall recalls, “the issue of time in stratigraphy did not begin to have a major influence on the science until Ager’s work in the 1970s” (Miall 2015, 285). In this section, I introduce a few of the key ideas from Ager’s *The Nature of the Stratigraphical Record* before turning to the most influential development in late twentieth century stratigraphy—the articulation of a new model of large-scale stratigraphic architecture based on seismic records of continental marine deposits.

What did Ager find so curious about the “stratigraphical record”? For one thing, “[the] record is spasmodic and ridiculously incomplete, with particular strata and fossils extremely widespread, but separated by vastly longer gaps than anything that is preserved” (Ager 1993a, 112). For another, individual strata and fossils are almost certainly diachronous (Ager agreed with Shaw that most sedimentary strata and fossils spread diachronously, with the possible exception of deep-sea oozes). Because the record is “ridiculously incomplete,” however, these strata and fossils are “to all intents and geological purposes synchronous”—it is not the case that diachronism destroys the value

¹² The book was *The Nature of the Stratigraphic Record*, and it went through three editions in Ager’s lifetime (1973, 1981, 1993a).

of fossils and sedimentary facies as markers of equivalent time. The death of lateral continuity, then, had been somewhat exaggerated. As unlikely as it must have seemed in 1973, the stratigraphic layer cake had some life in it still.

Ager himself disowns the layer cake analogy, but his picture of the rock column permits the recovery of a certain layer cake pattern. In Ager's view, one of the chief results of "Recent sedimentary studies has been the demonstration of lateral rather than vertical sedimentation" (87). "Modern deposits are not, it seems, laid down layer upon layer over a wide area," as old-fashioned layer cake stratigraphers liked to imply. Rather, "[they] start from a particular point and then build out sideways as in the traditional picture of a delta."¹³ To illustrate his point, Ager offers the analogy of "carpets being brought periodically into a shop for display and rolled out one by one on a pile" (112). The end product of this stacking would resemble a layer cake with clearly distinct layers; still, "the process of formation and the record it preserves [would be] different [from the naive layer cake view]." For one thing, "we know that the time-gaps between successive layers might have been very considerable"—an issue for any layer cake picture tied to the outdated model of continuous sedimentation (112–113). In addition, "[we] know that when a new layer arrived, it was not deposited simultaneously all over the preceding layer" (113). Ager's criticisms, then, are not directed at the layer cake pattern, but rather at the view that sedimentation resembles "gentle rain from heaven"—a view that he curiously associates with the notion of a facies mosaic (since God's rain falls on the just and the unjust, the muddy lagoon and the barrier reef).

¹³ The formation of a delta involves the deposition of sloping layers of sediment atop already-existing layers. It is an example of what geologists call "progradation": in other words, the building of a shore- or coastline into the sea.

Ager's most important contribution, however, was not his idea that sedimentation consists in lateral spreading instead of vertical stacking.¹⁴ Instead, it was his claim that significant portions of the geological record were deposited in a very short time by “catastrophic events” like hurricanes and underwater avalanches (termed “turbidity currents”). These events are instantaneous in geological terms, and may cover areas of hundreds or even thousands of square kilometers. As a consequence, they can be used as time-lines to correlate distant stratigraphic sections, even if most of the rocks comprising those sections were deposited diachronously. Writes Ager:

We have managed to confuse ourselves for years with the jargon of lithostratigraphy, biostratigraphy, chronostratigraphy and the rest. In fact, it can be argued that basically there are only two concepts—rocks and time—with the rest just an obfuscation of nomenclature. Nevertheless, it is useful to distinguish between our various means of correlation and I make no apology for suggesting another term, just to draw attention to its usefulness as a method. This is what may be called “event stratigraphy” in which we correlate not the rocks themselves, on their intrinsic petrological characters, nor the fossils, but [discrete geological and biological] events. (Ager 1993a, 98–99)

The term “event stratigraphy” has since passed into general usage; likewise the practice of constructing high-resolution time scales on the basis of “geologically instantaneous” events (Aigner 1985; Kauffman 1988; Einsele et al. 1991). According to Patzkowsky and Holland (2012), it is one of two approaches that lay at the heart of stratigraphic paleobiology, since it permits the high-resolution correlation of individual beds and sets of beds—a prerequisite for resolving the dynamics of rapid paleobiological events within

¹⁴ As Ager himself notes, this view had already been championed by Shaw (1964), among others.

single sedimentary environments (Holland 1999, 2000). (The other approach is sequence stratigraphy, the subject of Section 5.)

There is a flip side to the view that most sedimentation is episodic, and that large portions of the geological record accumulate in virtually instantaneous events. This is the notion that the record is ridiculously incomplete—“more gap than record,” as Ager puts it—even in putatively complete sections. Ager observes that the traditional way of representing the stratigraphic column is as a stack of rocks (or in a correlation chart, a series of stacks) interrupted, if at all, by minor gaps (see Figure 12). Yet Ager proposes that a “far more accurate picture” is that of “[a] long gap with only very occasional sedimentation” (Ager 1993a, 52–53).

Perhaps the best way to convey this [picture] is to remember a child’s definition of a net as a lot of holes tied together with string. The stratigraphical record is a lot of gaps tied together with sediment. It is as though one has a newspaper delivered only for the football results on Sunday and assumes that nothing at all happened on the other days. (Ager 1993a, 53).

As Ager notes in a later book, each bedding plane is effectively an unconformity—a surface corresponding to a period of non-deposition and perhaps erosion (Ager 1993b). And the number of bedding planes in a given stratigraphic succession is enormous. The upshot is that “gaps probably cover most of earth history, not the dirt that happened to accumulate in the moments in between.” Or as Ager writes in the oft-quoted conclusion to *The Nature of the Stratigraphical Record*: “the history of any one part of the earth, like the life of a soldier, consists of long periods of boredom and short periods of terror” (Ager 1993a, 141).

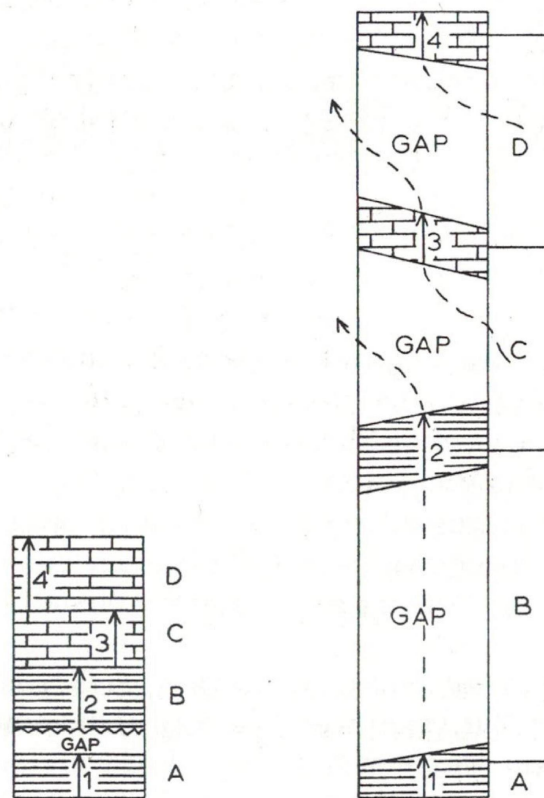


Figure 12 A comparison of a conventional representation of a stratigraphic section (left) with “what is probably the true picture” (right). Notice that in the “probably...true picture,” strata are represented at an angle, in accordance with Ager’s view that sedimentary accumulation consists in lateral spreading as opposed to “gentle rain from heaven.” (From Ager 1993a)

Ager’s work had an immediate impact on stratigraphic thought. Apart from stimulating interest in “catastrophic” events, perhaps the most important thing it did was reopen the question of the temporal scope of rock layers, and increasingly, of surfaces corresponding to gaps in the record. Time is continuous, but sedimentation—and therefore the stratigraphic record of time—is not. Yet to understand the distribution of gap-lengths and the meaning of surfaces corresponding to periods of non-deposition or

erosion, geologists required tools for interpreting the stratigraphic record in terms of controls on sedimentary accumulation over a range of spatial and temporal scales. These were mostly unavailable when the first edition of *The Nature of the Stratigraphic Record* appeared in 1973. They were soon to become available, however, with the advent of a new approach to stratigraphy for larger scales of time. This approach, which studies the architecture of stratigraphic sequences over a range of spatial and temporal scales, is termed “sequence stratigraphy.” Although little heralded outside the earth sciences, it is hard to overstate its importance for the study of sedimentary rocks. To a greater extent than even the “revolution” in lithostratigraphy, sequence stratigraphy was a game-changer.

5. Sequence Stratigraphy

The basic ideas behind sequence stratigraphy began to coalesce in the 1940s, but it was not until the 1980s that they precipitated what has been called a revolution in stratigraphy (Sloss 1988).¹⁵ The chief architect of this revolution was Peter Vail, a stratigrapher working for the Exxon oil company’s Upstream Research Group (of which more in a moment). However, it was Vail’s doctoral advisor, Laurence Sloss, who got the ball rolling with his study of the enormous packages of strata comprising the cratonic region of North America.¹⁶ Sloss was an outlier among American stratigraphers in the

¹⁵ This is different from the revolution in lithostratigraphy, which took place during the 1960s. Geologists, you will have noticed, are fond of the term “revolution.”

¹⁶ A craton is a large and ancient block of the earth’s crust that comprises the nucleus of a continent. A cratonic region is the region overlying a craton and containing piles of younger rocks. What Sloss and colleagues showed was that the cratonic region of North America could be subdivided into four “unconformity-bounded successions”—thick

middle of the twentieth century. While many of his colleagues were engaged in the mapping and analysis of small-scale stratigraphic units (as described in Section 3), Sloss was interested in “an entirely different scale of research, encompassing whole basins [and indeed multiple basins at once]” (Miall 2015, 288). Sloss was also unusual for his interest in external controls on sedimentary processes—things like tectonism and sea-level change—as opposed to processes internal to sedimentary systems. He speculated in 1949 that the North American craton contained four major “unconformity-bounded successions,” and that these were controlled by tectonic movements: large-scale movements of the earth’s crust (Sloss et al. 1949). When the crust went down, shallow seas invaded the continental interior and sediment was deposited; when it went up, the seas retreated, exposing the previously deposited rocks to erosion. Later Sloss demonstrated an association between these same “sequences” (the number had since grown to six) and major rises and falls in sea-level, suggesting that global sea-level exerts a control on sediment accumulation in sedimentary basins. It was observations like this, more than anything else, that set the agenda for future studies of depositional sequences, including Vail’s work at the Exxon oil company.

packages of strata inferred to have been deposited during the same interval of time. This number was later increased to six in a paper that many regard as the earliest example of the modern “sequence” concept in practice (Sloss 1963; see also Sloss 1988).

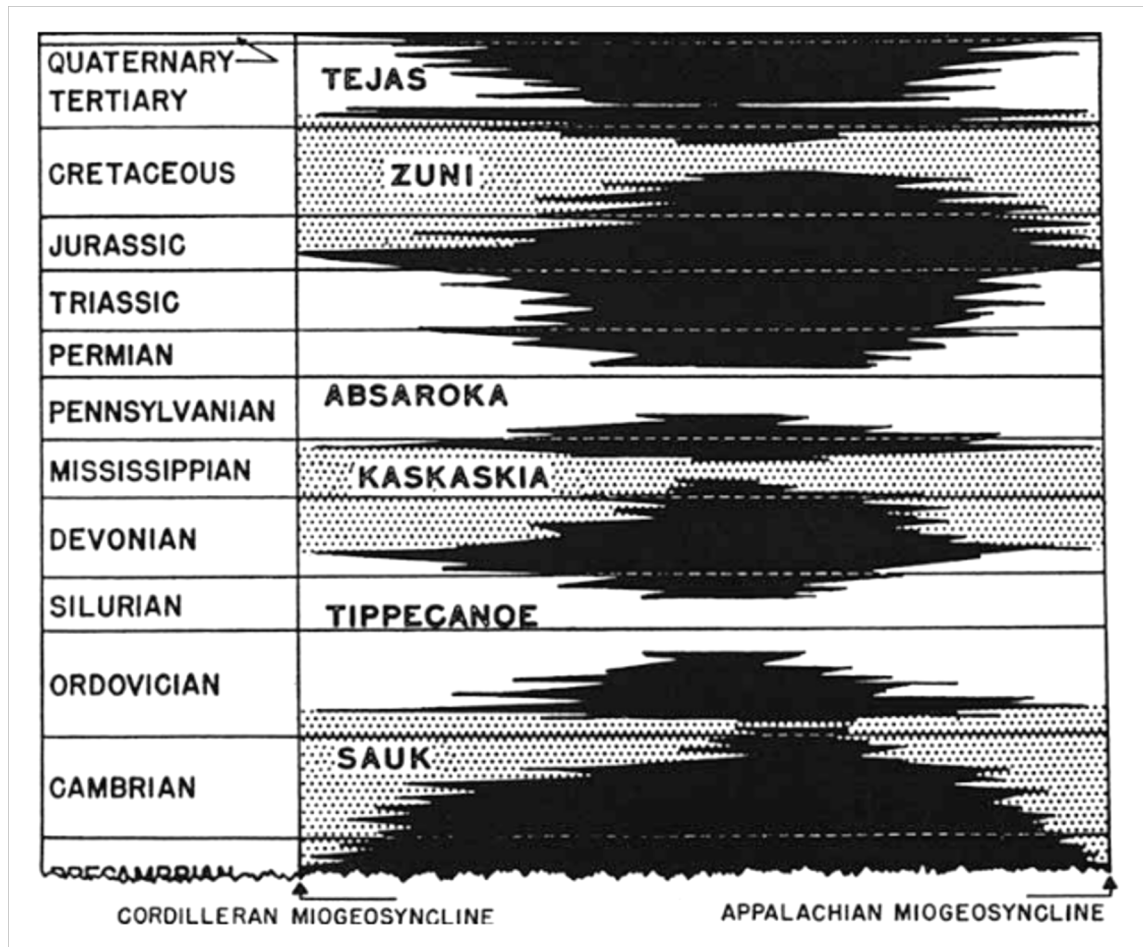


Figure 13 The six North American sequences of Sloss (1963). Each sequence is a package of strata that may be hundreds of meters thick in some places, and that represents tens to hundreds of millions of geological time. Moreover, each sequence is bounded by a major regional unconformity (or non-depositional surface) without specific time significance.

Vail began his career at Exxon interested in correlating Sloss-type sequences between basins using evidence from boreholes (Vail 1992). Yet when high-quality seismic reflection data became available during the 1960s (largely in virtue of advances in computer processing), he decided to switch tracks. Oil companies had begun using seismology to map underground stratigraphic relationships during the 1920s, hopeful of

locating oil and gas reserves without costly drilling. But the practice was notoriously unreliable, and remained so even as increasingly high-resolution reflection profiles became available. In Portuguese Guinea, for example, Vail was tasked with examining a series of three wells, the first of which had been drilled into “a major Cretaceous reservoir of sand, overlying an unconformity with Paleozoic rocks below” (Vail 1992, 86). The top of the sand layer corresponded to a reflection surface (a line on a seismic profile), and since it was assumed that seismic reflections were generated at facies boundaries, it was expected that a second well would encounter sand at about the about same level (i.e., at the depth corresponding to the same reflection surface). Yet when the second well was drilled it encountered sand two reflection layers lower than the first well. A third well encountered sand at even greater depth. This seemed to indicate that whatever was generating the reflection surface in this case, it was not a change in rock type associated with a facies boundary.

The breakthrough came when Vail recruited a paleontologist, Lou Stover, to date the sand and reflection layers using biostratigraphy. This revealed that the reflection surfaces—but not the sand layers—were equivalent in age. Vail’s earlier work had demonstrated that physical surfaces in rocks “cross the facies of time-transgressive rock units, suggesting that seismic reflections do not follow [lithofacies boundaries]...but instead [follow] the detailed bedding patterns or the real physical surfaces in the rocks” (Vail 1992, 87). Stover’s dates served to confirm this. The surfaces on reflection profiles correspond to isochronous physical surfaces as opposed to diachronous facies boundaries: they were time lines etched in stone. Far from “a low-resolution tool for mapping major rock units,” reflection seismology was in fact “a high-resolution tool for determining

chronostratigraphy [the relative ages of rocks].” For someone interested in stratigraphic correlation, a more welcome result could hardly have been imagined.

The discovery that seismic records can be used to reconstruct the temporal development of sedimentary basins was a revelation. Using Exxon’s wealth of proprietary data, Vail’s team set to work “[developing] the methodology to make regional chronostratigraphic correlations and to put stratigraphy into a geologic time framework for mapping and the understanding of paleogeography” (Vail 1992, 87). The seminal publication appeared in 1977, and is commonly cited as AAPG Memoir 26 (although its full name is *Seismic Stratigraphy—Its Application to Hydrocarbon Exploration*). In it, Vail and colleagues demonstrate that the stratigraphic record of continental shelves consists in a series of stratal packages partially bounded by unconformities, which the authors call “depositional sequences.” Because unconformities and their correlative conformities appear as lines on seismic profiles, it follows that seismology can be used to recognize depositional sequences throughout a sedimentary basin, making them a highly useful way of dividing up the stratigraphic succession. In addition, because the boundaries of sequences are presumed to have time significance, sequence stratigraphy gives researchers “a powerful methodology for the analysis of time and rock relationships in sedimentary strata,” blurring the line between chrono- and lithostratigraphy and reopening important questions about what controls the distribution of gaps and sedimentary environments in the rock record (Maliva 2016, 32).¹⁷

¹⁷ This is no place to review the complex history of sequence stratigraphy since the 1970s. Suffice it to say that the original model of Vail and others was repeatedly amended as higher-resolution seismic data became available, and as the importance of factors beyond eustatic sea-level change became more widely appreciated (Miall and Miall 2001; Embry et al. 2007). Deductive models also played an important role, for

Sequence stratigraphy is notorious for its difficult terminology and steep learning curve. Still, the basics of the approach can be summarized simply enough. In Andrew Miall's succinct formulation, it is "essentially [the] study of repetitive cycles of [sediment] accumulation followed by [gaps], at various time scales" (Miall 2015, 295). No less important, it is a framework for interpreting the stratigraphic record in terms of a small number of basic parameters, including rates of eustatic sea-level change, tectonic subsidence and sediment supply.¹⁸ Importantly, these parameters are related, with rates of tectonic subsidence and eustatic sea-level rise controlling the space available for sedimentation, or "accommodation [space]."¹⁹ Changes in accommodation, in turn, control the accumulation of sediment over periods of tens of thousands to millions of years. (On shorter time scales, sediment accumulation is dominated by the depositional events that Ager described as "catastrophic.")

Changes in accommodation also influence the distribution of gaps in the stratigraphic record. When the rate of sediment supply exceeds the rate of increase in

example, in clarifying the internal anatomy of sequences and the meaning of key surfaces (Jervey 1988; see also Posamentier and Vail 1988; Van Wagoner et al. 1988). A particularly important development was the ability to apply sequence stratigraphy directly to outcrops and well-logs, in the absence of seismic reflection data (Van Wagoner et al. 1990). All this was crucial in fashioning sequence stratigraphy into a tool for interpreting the depositional origin of rock bodies, and for predicting the heterogeneity, extent and character of constituent facies.

¹⁸ The term "eustasy," or eustatic sea-level change, refers to changes in global sea-level as distinct from more local variations (Dott 1992).

¹⁹ More precisely, "accommodation [space]" is defined as the vertical envelope between the sea surface and the basement of rocks beneath the sedimentary pile, which is available for potential sedimentation (Jervey 1988). Changes in accommodation reflect the sum of changes in eustatic sea-level and tectonism, with rising seas and tectonic subsidence increasing accommodation, and falling seas and tectonic elevation decreasing accommodation.

accommodation, sediment accumulates, forming packages of sediments termed “parasequences.”²⁰ Parasequences are successions of relatively conformable strata bounded at their tops by “flooding surfaces” associated with abrupt deepening events (often, periods of nondeposition). They are produced by oscillations in the balance between sediment supply and accommodation space; but these oscillations are superimposed on longer-term changes in eustatic sea-level that build the major features of depositional sequences (Figure 13). The most important of these features are “systems tracts,” which commonly consist of parasequences bounded by characteristic surfaces of various types. The names of the systems tracts are not important yet; but what *is* important is that they succeed one another in regular order, and that the ensemble of systems tracts is bounded at its top by an erosional surface called the “sequence boundary.” This is the surface that separates one depositional sequence from another. It forms when sea-level falls, promoting the erosion of sedimentary deposits. Typically, sequence boundaries represent significant unconformities, which record long periods of time in which no sediment accumulates. But they are not the only chronostratigraphically significant surfaces within a sequence, and other surfaces, like the “maximum flooding surface,” are also associated with periods of highly reduced deposition.²¹

²⁰ A typical parasequence is between one and ten meters thick, and represents tens to hundreds of thousands of years of elapsed time. By contrast, depositional sequences are considerably thicker (comprising multiple stacked parasequences), and typically represent millions of years of elapsed time.

²¹ To say that a surface is “chronostratigraphically significant” is to say that all the rocks overlying it are always younger than all the rocks underlying it. Because of this, chronostratigraphically significant surfaces can be used as markers for local correlation.

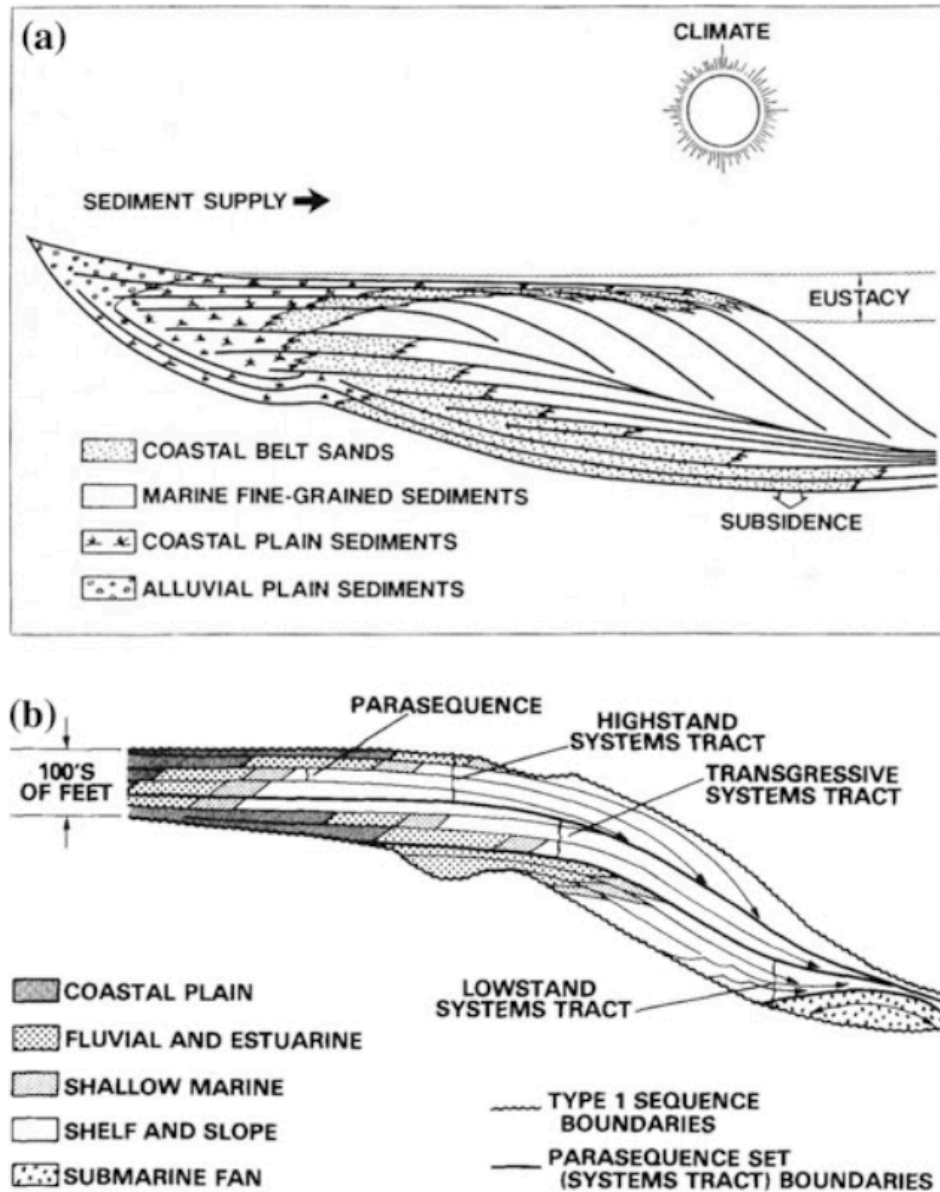


Figure 14 The anatomy of a depositional sequence. Here (a) shows a series of *parasequences*: packages of sediment exhibiting a gradual upward change in facies, bounded by flooding surfaces (dark lines). By contrast, (b) pictures the anatomy of the depositional sequence as a whole, comprising stacks of parasequences organized into discrete “systems tracts.” (From Mitchum and Van Wagoner 1991)

In addition to conferring an understanding of the nature and distribution of gaps in the record, sequence models are also informative about facies relationships. Consider

parasequences—common building blocks of depositional sequences. As noted, each parasequence records a cycle of decreasing and then increasing accommodation rate for a particular sediment supply. Because sediments entering the sea accumulate first in nearshore environments before building outward, parasequences record the seaward movement of a shoreline. This means that, if we examine a parasequence in cross-section, the facies within this succession will represent progressively shallower environments as we move from the bottom of the succession to the top (until we reach the flooding surface, at which point a deep-water facies will be juxtaposed with a shallow-water one). Likewise, if we examine whole sets of parasequences stacked one upon the other, it will be possible to discern trends in their constituent facies and three-dimensional arrangement, which tell of long-term trends in the physical environment (Van Wagoner et al. 1990).²² Sequence stratigraphy, then, permits a theoretical understanding of facies relationships within sedimentary successions, since sequences record a hierarchical set of paleoenvironmental variations in their anatomical structures. This in turn brings order to the apparently crazy-quilt pattern of sedimentary environments for a range of marine and terrestrial systems.²³

²² Trends in global sea-level are especially salient, and are recorded by different stacking patterns in parasequence sets. These patterns in turn allow stratigraphers to make inferences about long term rates of sedimentation and accommodation in a basin.

²³ At least one influential stratigrapher has even claimed that sequence stratigraphy rehabilitates a version of the layer cake metaphor, although this is controversial (Brett 2000; Brett et al. 2007). In sequence stratigraphy, the stratigraphic record is divisible into packages of strata bounded by unconformities and their correlative conformities. These packages are tapering wedges that shade off in every direction—so far, nothing resembling a layer cake. However, they also contain stratigraphically significant surfaces, which cut across local facies like “frosting layers” through a well-marbled cake (Brett 2000). Many beds representing catastrophic events also cut across local facies, and for this reason can be used to subdivide the rock record into approximately time-parallel

These innovations had far-ranging effects on geological practice. Perhaps the most important was to transform the oil and gas industry, enabling geologists to predict stratigraphic relationships in areas that had yet to be explored by costly drilling (Maliva 2016). But the approach had some positive consequences as well, including that it: (1) “[raised] questions about eustasy and tectonics that had largely been forgotten in the desire...to explain everything as just so much facies shifting”; (2) “[recognized] genetic units...that have meaning in the interpretation of earth’s history”; (3) “[emphasized] the incompleteness of the record and [opened] up the study of surfaces, those recorders of times of nondeposition and erosion that represent most of earth history”; and (4) “[raised] questions about sediment accumulation over much larger timescales than sedimentology has traditionally dealt with and about secular trends in the nature of sediment accumulation” (Holland 1999, 412). All these were relevant to paleobiological research, as Steven Holland was eager to point out. And fittingly, it was Holland, as much as anyone else, who leveraged the distinctive features of the new stratigraphy for

units. A kind of chronostratigraphic “layer cake” can thus be recognized, composed not of tabular layers (as in the old layer cake picture), but rather of tapering wedges penetrated by isochronous “frosting layers” that provide “high-resolution markers with which to subdivide the stratigraphic record into approximately time-parallel units” (Brett 2000, 497).

It should be noted that many stratigraphers believe this stretches the layer cake metaphor beyond all recognition and usefulness. In the words of Steven Holland, “[the layer cake metaphor] runs completely counter to how sequence stratigraphers envision the rock record. To make the metaphor square with the rock record as we now know it, the cake has to be modified so that it looks nothing like any cake you’ve ever been served...[The] basic point...[is] that there are some lithologically based horizons that have chronostratigraphic usefulness...[but to describe this in term of cake layers] is a distraction, and a misleading one in terms of how the rock record is put together” (Holland, personal communication).

paleobiological ends, most notably in a pioneering model of stratigraphic architecture and fossil preservation (Holland 1995a).

6. The Stratigraphic Distribution of Fossils

As I observed in Section 2, paleontologists have long been exercised by worries about their data. In the wake of the Paleobiological Revolution, this led to efforts to construct increasingly comprehensive databases, which could, “in idealized form, claim to represent the complete fossil record” (Sepkoski 2017, 402). It is hard to overstate the importance of these databases for the history of the field; research on large datasets has illuminated topics ranging from patterns of diversification and extinction (Raup and Sepkoski 1982; Sepkoski 1984; Benton 1985), to changes in biomass and guild occupation (Bambach 1993), to onshore–offshore patterns of clade origination (Jablonski et al. 1983; Jablonski and Bottjer 1991). However, for studies at the scale of outcrops to depositional basins, global taxonomic databases are of more limited utility. Here what is needed are better tools for analyzing the stratigraphic distribution of fossils, and for dissecting the incompleteness of the fossil record into component biases (e.g., facies control, condensation and missing time).

It is the great virtue of sequence stratigraphy that it provides these tools for many stratigraphically important settings. Yet this was not immediately apparent, and in the years following the publication of AAPG Memoir 26 (1977), the approach made only limited inroads into paleontology. Sequence analysis was first applied to the study of sea-level changes, where information from fossils was used to recognize and interpret sequence patterns and sedimentation dynamics (Hallam 1992; Brett 1998). Later it played

a role in building integrated models of depositional environments and paleoecology, like Susan Kidwell's influential models of shell accumulation (Kidwell 1986, 1991). Carlton Brett and colleagues applied sequence stratigraphy to the Devonian rocks of New York beginning in the 1980s, enabling them to study temporal variation in fossil communities (Brett et al. 1990; Brett 1992).²⁴ Prior to the mid-1990s, however, explicitly paleobiological applications of sequence stratigraphy remained a relative rarity.

This began to change during the second half of the 1990s, for reasons having to do with the internal development of the field. Around this time, self-identified paleobiologists were increasingly turning from the museum stacks to the outcrop—to studies that required the collection of fossils in well-resolved temporal and environmental frameworks. From these projects, “a new category of research questions” began to emerge “between the ‘traditional’ research avenues of paleobiology and field-based paleontology.” According to Mary Droser, these questions “are the domain of ‘field [or stratigraphic] paleobiology’,” and include:

“Does this turnover correspond with a significant relative sea-level change?,” Can we refine the timing of this radiation?,” “Do these clades actually radiate in concert?,” “Was this extinction gradual or sudden?,” “How significant is environment in morphological variability?,” “Does a basinal pattern reflect taphonomic biases?,” “Can taphonomic biases be corrected for?,” [and] “Can we test patterns of radiations through examination of proxies?” (Droser 1995, 507–508)

Droser is careful to note that these new questions are not the province of any particular

²⁴ These studies resulted in the characterization of “coordinated stasis,” a phenomenon in which “groups of coexisting species lineages display concurrent stability over extended intervals of geologic time separated by episodes of relatively abrupt change” (Brett et al. 1996, 1; see also Brett 2012).

methodological approach; yet she also observes that “the integration of stratigraphy with paleobiology is one of the major breakthroughs in the analysis of the fossil record and is an important approach in field paleobiological studies” (Droser 1995, 508). In particular, the integration of sequence and event stratigraphy with paleobiology can be expected to yield important insights into the history of life and its interaction with factors like sea-level change, as several earlier authors had suggested (e.g., Kauffman and Sageman 1992; Gómez and Fernández-López 1993; see also Brett 1998).

But this is not all. In addition to providing high-resolution frameworks for interpreting events in the fossil record, sequence stratigraphy promised to illuminate biases in the record as well. These biases had come under increased scrutiny since the 1970s, as research into fossil preservation had expanded, first in Germany, and then in the United States (Behrensmeyer and Kidwell 1985). Known as “taphonomy,” the science of fossil preservation held great interest for paleobiology, since many of the biases that affect the fossil record are the result of the selective preservation of organic remains, or else of processes that influence the fate of biological materials after burial. However, not all biases that affect the fossil record are the result of selective preservation or postmortem tinkering. Others are the result of “the *selective archiving* of the sedimentary deposits that entomb those remains,” and these are the province of stratigraphic geology (Kidwell and Holland 2002, 562, emphasis added). In particular, they are the province of sequence stratigraphy, since they concern the distribution of sedimentary environments in the rock record, and the processes that structure the available record in space and time.

It is this second kind of bias that supplies the focus of Steven Holland’s study,

which appeared in the journal *Paleobiology* in 1995. Bearing the unassuming title, “The Stratigraphic Distribution of Fossils,” the study was a first-of-its-kind attempt to apply computer simulation to the problem of what controls the distribution of fossils in sedimentary basins.²⁵ The problem is of wide relevance, since many paleontological practices involve the documentation and interpretation of fossil occurrences in outcrops and sedimentary basins. Still, Holland’s aims in this article are straightforwardly paleo-*biological*, as his opening remarks make plain:

Sequence stratigraphy has revolutionized stratigraphic analysis in much the same way that facies models did decades ago. Many paleobiological and biostratigraphic models require or use stratigraphic testing; these models include biozonation studies, mass extinction studies, and confidence limits on ranges. Many other paleobiological concepts are based, at least in part, on the distribution of fossils in the stratigraphic record; such concepts include punctuated equilibrium. Therefore, any fundamental change in stratigraphic thought should require a similar reexamination of paleontological thought. (Holland 1995a, 92)

In pursuing this reexamination, Holland identifies three factors as both significant in their effects on fossil distribution and amenable to quantitative modeling. These are: (1) the rarity of fossils (or the chances that a fossil taxon will be collected in a bed where it is expected to occur); (2) facies control (or the probability of collection for a taxon as a function of an environmental variable, like water depth); and (3) sequence architecture (which controls facies change and sedimentation rate over space and time). Altogether, they provide an integrative picture of the controls on fossil occurrence—a picture that

²⁵ Holland produced two modeling studies in 1995: the first (Holland 1995b) appeared in an edited collection, and contained the first three of the four models presented in Holland (1995a). Holland (1995a) is the first publication to contain Holland’s complete sequence model (the fourth model).

both generates predictions about what will be observed in outcrop and suggests strategies for field studies of paleobiological events.

Holland presents his model in four “steps,” beginning with a model of “the perfect stratigraphic record” and then layering in additional factors to increase its realism. In the first step, Holland assumes that any taxon living in a sedimentary basin at a time will be preserved in a bed deposited at that time. So, because the “probability of collection” is here 100%, the stratigraphic distribution of fossils will perfectly mirror the true durations of fossil taxa. Step two complicates the situation by simulating probabilities of collection of 50%, resulting in a record strewn with gaps, and 10%, resulting in a record that is “more gap than record,” to recall Ager’s memorable phrase. However, because the model lacks any representation of ecology, the preserved fossils are scattered randomly throughout the range of a taxon. It is this simplification that the third model seeks to remove.

Holland’s third model begins with the observation that “[m]any, if not most, taxa are most abundant at some particular level of an environmental variable” (Holland 1995a, 94). So in marine environments, most taxa show a peak abundance at a particular point along a depth gradient, with population numbers shading off as you move away from this optimal water depth (Ziegler 1965; Ziegler et al. 1967; Bretzsky 1969; Cisne and Rabe 1978).²⁶ Since fossil abundance is related to the probability of collection at a bed (McKinney 1968), it is possible to model the probability of collection for nearshore marine taxa as a function of water depth. And this is exactly what Holland’s third model

²⁶ This is not because water depth governs the distribution of organisms directly, but rather because it is correlated with many variables that do directly govern their distributions (including temperature, substrate consistency and salinity).

does, by specifying preferred depths, depth tolerances and peak abundances for simulated taxa.²⁷ Together, these variables comprise the form of facies control for the model, where “facies control” refers to the influence of an environmental variable—in this case, water depth—on the distribution of fossil taxa.

Facies control is one of the complicating factors added to model three. In addition, the model includes a representation of water depth change for the simulated section (generating facies change). This is modeled as a series of two shallowing upward cycles bounded by abrupt deepening events, matching the pattern of two stacked parasequences. With these two factors in the mix, the model generates a number of distinctive results. In particular, “[the] combination of [water depth change] and facies control produces a characteristic fossil distribution consisting of a few sporadic occurrences low in the parasequence, followed by a zone in which the fossil achieves a peak abundance, followed by a return to sporadic occurrences” (Holland 1995a, 96). That is, simulated taxa display a “scarce-common-scarce” pattern of occurrence within a parasequence (Holland 1995b), and a clustered and nonrandom distribution over the entire section. Parasequence boundaries (corresponding to flooding surfaces) truncate this pattern when a taxon’s preferred environment lies close to the parasequence boundary. Importantly, this effect is produced solely by facies change—no depositional gap is required to truncate the ranges of facies controlled taxa. The message for paleobiologists is accordingly a cautionary one: “The abrupt disappearance of a taxon is likely to represent a true extinction where it occurs in the middle of a parasequence, but probably

²⁷ Preferred depth is the optimal water depth for a taxon. Depth tolerance is the sensitivity of a taxon to water depth, measured as the standard deviation of the abundance/depth distribution. And peak abundance is the abundance of a taxon at its preferred water depth.

represents [mere] facies control when it occurs at a parasequence boundary” (Holland 1995a, 96).

To interpret ecological and evolutionary processes in sedimentary basins, however, one must understand the effects of stratigraphic architecture on fossil occurrences. For this, a complete sequence model is required—one that enables the incompleteness of the fossil record to be dissected into component effects. This is what Holland’s fourth model attempts to provide. The model has two main components. The first is a model of ecology, in which 1,000 taxa are assigned facies characteristics from uniform probability distributions. At each time step in the simulation (corresponding to 50,000 years of elapsed time), a taxon has a fixed probability of going extinct. If a taxon goes extinct, a new taxon is created for the next time step with randomly generated facies characteristics; so total diversity does not vary over the length of the simulation. This means that observed patterns are unlikely to be the result of a surplus of originations relative to extinctions for a time interval (or vice versa). Most likely, they will owe to the filtering effects of the stratigraphic record.

The second component of the model is a representation of environmental change and sedimentation. These features are simulated using existing models of sedimentary basin filling, in which accommodation space is generated through a combination of tectonic subsidence and eustatic sea-level change, and sediment is deposited within this space according to a diffusion function. Holland’s model simulates two complete depositional sequences, each composed of three systems tracts: the “lowstand systems tract” (LST), “transgressive systems tract” (TST) and “highstand systems tract” (HST). The TST consists of two parasequences; the HST, of six. (No sediment is deposited in the

LST.) As in the third model, parasequences are modeled as shallowing upward cycles of deposition bounded at their tops by abrupt deepening events recorded by flooding surfaces.

The process of sedimentary basin filling proceeds as follows. At the base of each sequence is a surface known as the “sequence boundary,” which forms when relative sea-level is falling. During this interval, no new sedimentation takes place, so the sequence boundary corresponds to a gap in the rock record. When relative sea-level begins to rise, deposition is renewed and parasequences stack seaward in a net shallowing pattern. These parasequences form the “lowstand systems tract” (LST), so named because it sits at a topographically lower position than the rest of the sequence. As relative sea-level rise accelerates (such that the rate of sea-level rise exceeds the rate of sedimentation), the pattern of stacking is reversed and parasequences exhibit a net deepening trend: the transgressive systems tract (TST). Separating the LST and the TST is a flooding surface called the “transgressive surface.” This marks the point at which seaward stacking is replaced by landward stacking, and is often associated with reduced sedimentation (a phenomenon known as “[stratigraphic] condensation”). Other flooding surfaces within the TST may also exhibit considerable condensation. Finally, as the rate of sea-level rise begins to slow, parasequences again start to build seaward, exhibiting a net shallowing trend. At this juncture too there is a flooding surface—the “maximum flooding surface”—which records the greatest water depth in the sequence and is often highly condensed. The parasequences deposited atop the maximum flooding surface comprise the “highstand systems tract” (HST). These are bounded at their top by the sequence

boundary, which, to reiterate, forms when relative sea-level is falling.²⁸

Holland's fourth model simulates two depositional sequences, each with a duration of 3.5 million years. It has three steps. First, a sedimentary basin is generated using a basin simulation. Then, a suite of species is produced, each characterized by a set of randomly generated facies characteristics. Finally, occurrences of each taxon are simulated within the sedimentary basin. For each horizon across the basin, the age and water depth of the horizon are used in conjunction with the environmental parameters to determine the probability of collection for a species (see Patzkowsky and Holland 2012). This probability is then compared with a random number generator to test for the occurrence of a species at a horizon, and a list of occurrences is compiled. From this a record of first and last appearances can be generated, and the number of first and last appearances per section plotted for each section in the depositional sequences.

The results of the simulation are striking. To begin, it is immediately apparent that first and last occurrences (FADs and LADs, respectively) are concentrated at particular stratigraphic positions, not randomly distributed as they would be if the fossil record were uncontrolled by sequence stratigraphic architecture.²⁹ FAD spikes are particularly well developed at the four TST flooding surfaces, for example, including the two transgressive surfaces (which are highly condensed). These spikes are dominated by shallow-water and environmentally-tolerant taxa that originated during the lowstand (when no sediment was

²⁸ This was how the HST was defined in 1995, at any rate. Today, it is customary to recognize a fourth systems tract, the "falling-stage systems tract" (FSST), which separates the HST from the sequence boundary (making the top of the HST a basal surface of forced regression, at least in those places where the FSST is present).

²⁹ FAD and LAD stand for "first appearance datum" and "last occurrence datum," respectively.

deposited), or else by deep-water and environmentally-picky taxa that originated during the LST or the shallow-water portion of the previous HST (when preservation of deep water facies was not occurring). Importantly, the spikes are *not* the result of any biological response to sea-level change, since this is not possible within the model. Rather, they are produced by changing conditions of preservation, and are therefore reflections of the way the fossil record is physically assembled.

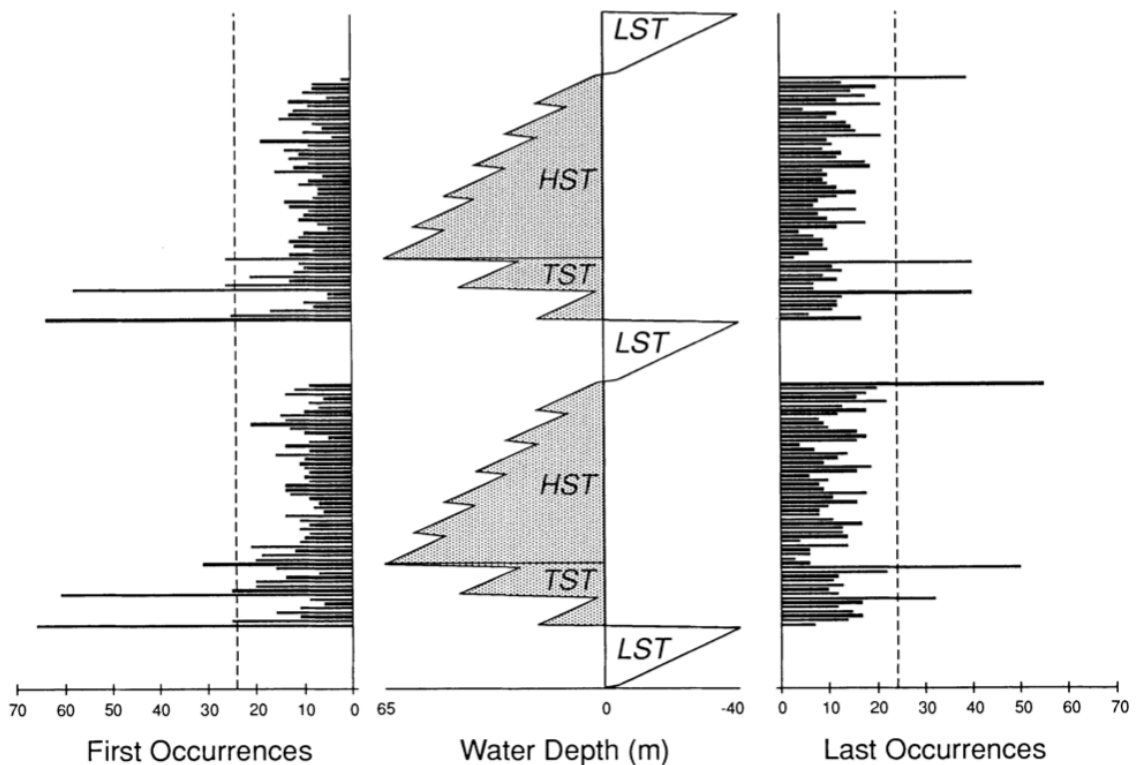


Figure 15 Results of Holland's complete sequence model, showing numbers of first and last occurrences for single stratigraphic sections throughout two depositional sequences. Notice the major spikes in first occurrences just above the sequence boundary and all TST flooding surfaces, and the major spikes in last occurrences just below the sequence boundary and all TST flooding surfaces. (From Holland 1995)

In a similar vein, LADs are concentrated immediately beneath the sequence boundary and above the flooding surfaces of the TST. The peaks at the sequence boundary represent the last occurrence of species that went extinct during the hiatus represented by the boundary, or else during the subsequent lowstand when no sediment was being deposited. The peaks at the flooding surfaces, by contrast, represent the last occurrence of shallow-water species that became extinct after the deepening events recorded by the flooding surface. It is notable that no spikes in either FAD or LAD are located within the HST, probably because no major floodings take place during the highstand, when sea-levels are falling.

In the remainder of his article, Holland shows that the simulation of certain taphonomic and ecological gradients modulates but does not change the stratigraphic position of these peaks.³⁰ This indicates that sequence architecture can predict where the fossil record of sedimentary basins is likely to be particularly misleading—a prerequisite for designing better sampling and analytical strategies. As noted, sequence boundaries are particular hotspots, with clusters of LADs expected *beneath* the boundary and clusters of FADs expected *above* it. This is an example of an unconformity effect, since the major factor generating the bias is unrecorded time. Similarly, LADs will cluster beneath transgressive flooding surfaces and FADs above them: a symptom of facies control, since it arises from the limited facies tolerance of taxa or the limited availability of facies in an outcrop. It should be noted that clusters of first and last appearances do not necessarily reflect the shaping of the fossil record by the rock record. Some of the most interesting

³⁰ These include gradients in preservation potential (associated with water depth), onshore/offshore diversity level, and environmental tolerance (again, associated with water depth)

events in life's history involve spikes in rates of origination or extinction; one need only think of the mass extinctions that commanded so much paleobiological interest during the 1980s and 1990s (Sepkoski 2021). Still, to recognize biologically meaningful spikes, knowledge of sequence stratigraphic architecture is useful, and in many cases, necessary (Patzkowsky and Holland 2012). Here is the ultimate utility of Holland's model: that it "reveal[s] not only ways in which the fossil record may be controlled by processes of sediment deposition, but also clues to recognizing those effects and strategies for overcoming them" (Patzkowsky and Holland 2012, 111).³¹

In this way, sequence stratigraphy helps to unlock the potential of the fossil record, and to extract biological signals from a fossil record shaped as much by stratigraphic processes as by biological history. In addition, it provides an approach to stratigraphic complexity that avoids Darwinian pessimism about the quality of the record, on the one hand, and the "dangerous seduction" of more literal approaches, on the other

³¹ Holland has continued to refine his model since 1995, frequently in collaboration with Mark Patzkowsky (Holland and Patzkowsky 1999, 2002, 2015; Holland 2020). Particularly important was a 2002 article, in which a more detailed model of sedimentation permitted the simulation of "range offset" (the difference in age between a taxon's first/last occurrence in a local section and its first/last occurrence in the basin as a whole). Simulations showed that range offset was much greater than has often been expected—on the order of one million years for marine invertebrate species. Given that the average estimated duration of marine invertebrate species is just four million years, this suggests that many species "are never sampled in any local stratigraphic record because their duration is so short that their preferred environment never occurs locally during their existence" (Patzkowsky and Holland 2012, 99). This in turn indicates that the temporal resolution of the marine fossil record may often be on the order of one million years—a level of resolution routinely exceeded by geochronologists. The upshot is that "the absolute age of a [rock] horizon may be known to a higher degree of precision than the timing of an extinction of a fossil found in that horizon" (Patzkowsky and Holland 2015, 100). Failing to take this into account can lead to major errors in the interpretation of extinction patterns (see Section 7).

(Holland 2000, 148). Unlike “generalized rereading,” this approach is capable of supporting inquiry at the scale of outcrops to sedimentary basins—scales in which field context plays a key role in the interpretation of fossil patterns.³² And because the effects of stratigraphic processes are “widespread and nearly inescapable for shallow marine and terrestrial settings,” the consideration of field context is not a mere luxury for studies at these scales (Patzkowsky and Holland 2012, 108). Instead, it is a vital part of the expansion of paleobiological inquiry, and of attempts to bridge the scales of “traditional” paleobiological and field-based paleontological inquiry. This expansion, and the cultural reconfigurations it engendered, is the subject of the final section.

7. Paleobiology, Prestige and “the Field”

From its consolidation in the Paleobiological Revolution, the science of paleobiology has had an ambiguous relationship with “the field.” All paleontology is ultimately based on fieldwork, yet this does not mean that every paleontologist—or even every group of paleontologists—must labor to produce new fossil collections. Indeed, a major accomplishment of the Paleobiological Revolution was to show how much damage a paleontologist could do with just a computer and a library card. Half-jokingly, it was said of Jack Sepkoski that his field site was the library; but this was no censure spoken under the breath of dusty fieldworkers. On the contrary, it was an honest description of a mode of practice that revolutionized paleontology, and that came to be associated with

³² “Field context” refers to the contextual features of geological data, and may include “three-dimensional position, vertical and lateral stratigraphic relations, the nature of bedding and bed contacts, the nature of surrounding lithofacies, observable sedimentary structures, fossil content, ichnofacies, and so on, at [various] scales” (Miall 2015, 273).

paleobiology in particular (Sepkoski 1993). All this suggests that to understand the evolution of paleobiology, including its expansion after the Paleobiological Revolution (ca. 1985–), some reflection on its relationship to “the field” is required.

In *Landscapes and Labscapes* (2002), Robert Kohler explores the border between “the lab” and “the field”: a lively and contested space structured by an overarching normative regime. Within this regime, labs are special places precisely because they are place-less; they are generic places, whose seeming universality provides “[a] symbolic guarantee that the science done there is everyone’s, not just someone’s in particular” (Kohler 2002, 7). “The field” inverts this logic. Natural spaces are irredeemably particular and variable—that is what makes them interesting objects of study, in addition to sites of knowledge-production. Yet this creates a problem for field scientists, who must contend with the notion that their knowledge is less than fully scientific. Comparisons of field scientists with stamp collectors underscore the greater value that attaches to the universal over the particular. In this culture, any system that escapes the undertow of particularity is likely to be highly valued; laboratory science is just the most obvious example. But laboratories are not the only place-less places in modern science, and in recent decades, another (non-)place has become equally important. This is the inside of a digital computer, which promises not a “view from nowhere,” but rather a “view from everywhere,” capable of synthesizing local observations into a consistent and fully synoptic data picture.

Kohler’s framework provides a useful way of analyzing the history of paleobiology, including its initial constriction and later expansion. Recall that during the Paleobiological Revolution, a small group of paleontologists sought to “[reinvent] their

discipline by creating a new identity for themselves” (Sepkoski 2012, 3). The identity was that of a modern evolutionary scientist; but no less important, it was that of a *non*-field geologist. Since its origin as a disciplinary specialty, invertebrate paleontology “had been an almost exclusively field-oriented science,” with biostratigraphy and paleoenvironmental analysis as major activities (Sepkoski 2012, 389). As Peter Ward recalls of the time before the Paleobiological Revolution: “More often than not, a hopeful new paleontologist, arriving in some professor’s office, would be sent away with some assigned geological quadrangle to map, or some quarry to excavate in the hopes of finding one more new species, or [orders to refine] some stratal sequence of time” (Ward 1994, 114). This was honest work, and led to a large number of invertebrate paleontologists becoming employed by the petroleum industry. Yet as Gould grumbled in 1980, these “non-biological approaches have not infused our profession with the excitement of ideas” (Gould 1980, 98). They also did few favors for paleontology’s reputation, which by the eve of the Paleobiological Revolution was in a sorry state.³³ In short, paleontology in the 1960s was in dire need of an overhaul, and the young Turks of the Paleobiological Revolution knew how to overhaul it: by shedding their associations with field geology, and using the power of digital computers to range god-like over the entire history of life.

This “view from everywhere” played the same epistemic role that the fiction of placelessness plays in the lab sciences. Whereas the field is a place of untrammelled particularity, large electronic databases abstract from this particularity to render the fossil

³³ Witness the gibe in a 1969 issue of *Nature* that “Scientists in general might be excused for thinking that...most paleontologists have staked out a square mile for their life’s work” (Anonymous 1969, 903).

record in a completely general way. Earlier non-electronic databases, like those compiled by Jack Sepkoski, abstracted even more, partially as a result of the massive “data friction” involved in compiling synoptic datasets by hand.³⁴ For example, Sepkoski's compendia tabulated only the first and last occurrences of taxa, with no data on geographical range, taxonomic richness, taphonomy, abundance or paleoecology. The result was a powerful but limited resource, capable of generating synoptic pictures of diversity through time, but incapable of answering questions that involve these unrecorded particularities (Marshall et al. 2018). (More recent databases record a much richer set of metadata, although missing metadata, especially for older collections, remains a serious issue.)

It is against this backdrop that the novelty of “stratigraphic paleobiology” is best understood. Stratigraphic paleobiology did not just represent the extension of paleobiology's model-driven approach to problems at the scale of outcrops to sedimentary basins. In addition, it represented an openness to inputs from stratigraphic geology that cut against the grain of the Paleobiological Revolution—or at least its rhetorical program. One dimension of this openness was conceptual, and involved the uptake of ideas about processes that structure the stratigraphic record in time and space. But no less important was a practical dimension, which involved a willingness on the part of paleobiologists to undertake painstaking stratigraphic work to better characterize the field context of fossil collections. Steven Holland, for example, did not just build the first integrated model of sequence stratigraphy and paleoecology (an expression of the first

³⁴ “Data friction” refers to “the costs in time, energy and attention” required to “collect, check, store, move, receive and access data” (Edwards 2010, 84). It is particularly great when data must be transferred from one medium to another.

dimension of openness). He also established the standard subdivision of Middle and Upper Ordovician strata into depositional sequences for the eastern United States. For this work, he was awarded the James Lee Wilson Award from the Society for Sedimentary Geology (SEPM)—a dubious honor for a paleobiologist, one might think, but only on the misleading assumption that one must be either a stratigrapher or a paleobiologist.

This twofold openness is stratigraphic paleobiology's unique contribution to the expansion of the discipline. More specifically, its contribution consists in a synergy between the two dimensions of openness, in which ideas from stratigraphy inform modeling work that in turn informs strategies of data collection and analysis (Holland 2000). On the data collection side, sequence analysis facilitates the collection of data in a time-environment framework, which allows fossils to be placed in context, “both for their habit and their sequence stratigraphic position” (Patzkowsky and Holland 2012, 108).³⁵ This allows researchers to assess the degree to which a continuous fossil record can be obtained for a particular environment, since empty cells in a time-environment matrix correspond to missing information (either because samples have not been obtained or because the relevant environments have not been preserved). In addition, time-environment sampling informs the interpretation of fossil occurrences, since missing information may scramble or distort the pattern preserved in outcrop. Regarded literally, a pattern may seem to indicate that a taxon disappeared abruptly at a particular juncture

³⁵ In time-environment sampling, temporal bins are provided by sequence stratigraphic analysis, and environmental bins are provided by field analysis of sedimentary facies. One benefit of this method is that it avoids the potentially circular method of using fossils to construct temporal frameworks that are in turn used to interpret patterns of fossil occurrence.

in a depositional sequence. Yet if that taxon has a strong facies preference, and if the relevant facies is not preserved in the beds surrounding that juncture, the pattern may be deceptive. In particular, if the juncture is a sequence boundary or flooding surface, models like Holland's caution against a literal interpretation of the data. Here field context earns its keep. By characterizing the field context of fossil collections, paleobiologists gain important insights into the resolution of their data, as well as valuable checks on literal readings of the fossil record.

Mass extinctions provide a useful example. Beginning in the 1980s, the study of mass extinction rose to occupy a central place in the disciplinary agenda of paleobiologists (Sepkoski 2021). Particularly important was determining whether an extinction had been gradual or rapid, since rates of extinction hold a key to determining causation for these episodes. Then as now, rates of extinction were evaluated by scrutinizing the timing of last occurrences in the stratigraphic record. Clustered LADs at particular stratigraphic positions were indicative of catastrophic mass extinction; smeared out extinctions, by contrast, were indicative of something more gradual. Of course, there were complications. Relatively early in the vogue for mass extinction, it was realized that even catastrophic mass extinctions will appear gradual when probabilities of collection are sufficiently low (Signor and Lipps 1982). This was important, since it had long been assumed that major extinctions were protracted affairs, spanning perhaps ten million years or more. However, if even rapid mass extinctions may appear gradual because of stratigraphic factors, then many extinctions may be veritably catastrophic despite leaving ambiguous signatures in the fossil record.

Recent decades have seen increasing support for models of catastrophic mass extinction. To take one example, the end-Permian mass extinction was once thought to have taken place over a period of perhaps ten million years, as the scattered continents slowly congealed into the supercontinent of Pangea (Erwin 1994; Ward 2004). Yet recent estimates place the duration of the extinction at less than 100,000 years, and perhaps considerably less (Burgess et al. 2014). This is a remarkable change, aided by the recognition of widespread “Signor-Lipps effects.” However, if recent simulations of the stratigraphic expression of mass extinctions are correct, this interpretation may need to be revised. The reason is that the stratigraphic pattern of last occurrences for the end-Permian “is considerably complicated by sequence stratigraphic architecture” (Holland and Patzkowsky 2015, 915). In the three geographical regions where Permo-Triassic (P–Tr) boundary sections are well developed, the main cluster of LADs is associated with at least one major flooding surface. In addition, all major P–Tr sections contain either subaerial unconformities beneath the main cluster of LADs *or* highly condensed strata in the earliest Triassic. All of these features are capable of concentrating last occurrences independent of biological processes; hence “[it] is likely that the appearance of the end-Permian extinction is considerably altered by stratigraphical architecture, with the extinction taking place over a substantially longer period...than generally thought.” Similar considerations apply to many other marine extinctions in the fossil record, with the notable exception of the K–Pg extinction (which also eliminated non-avian dinosaurs). This suggests, at least, that the stratigraphic expression of mass extinctions is more complicated than it is widely held to be, and that more attention must be paid to

stratigraphic architecture to resolve the tempo and biological nature of major extinction events.

In a certain sense, this is nothing new. Paleontologists have long been engaged in characterizing the spatial and temporal distributions of fossils, in many cases for biological purposes (Sepkoski 2012, 2021; Kelley et al. 2015; Tamborini 2017; Sepkoski and Tamborini 2019). Further, in some areas of paleontology (like paleoecology), close attention to field context and sampling strategies have traditionally been regarded as necessary features of responsible practice (Ager 1963; Raup and Stanley 1971).

Stratigraphic paleobiology is a latecomer in this respect, and amounts to something less than a full-blown (second) paleobiological revolution. Still, as Droser observed in 1995, “[the] wedding of [new] stratigraphic approaches with all fields within paleontology has made a significant impact on how we view the fossil and stratigraphic record. Essentially, we now have a stratigraphy within which we can frame life’s history, ranging from the scale of global correlation to the outcrop. We no longer view events that occur on a specific time scale in the fossil record without first asking what the relationship is to sea-level changes” (508). In addition, paleobiologists now increasingly evaluate patterns of fossil occurrence in light of stratigraphic processes like sediment accumulation. These are significant advances, which only came about because a group of paleobiologists was willing to traverse the cultural gap that had opened between paleobiology and stratigraphy during the Paleobiological Revolution. In so doing, they helped to reconfigure the cultural landscape of paleobiology, giving a new impetus to fieldwork and dissolving, at least in part, the barrier that Gould and others had raised between the “old” (field) and the “new” (evolutionary) paleontology.

8. Conclusion

Pessimism about the quality of the fossil record runs deep in paleontology. As Darwin was keen to emphasize, the fossil record is incomplete—“ridiculously” so, to crib a phrase from Derek Ager. This was welcome news, since it provided an apology for the failure of contemporary naturalists to uncover those transitional forms that must have existed in multitudes were his theory of gradual divergence correct (Sepkoski 2012). Later paleontologists mostly followed Darwin in regarding the record as deficient: as a book whose pages are “badly torn and blotted” (Cushman 1938, 356), or else as a document of which only “[o]ccasional lines from disconnected paragraphs in obscurantist chapters...can be read” (Pretorius 1973, quoted in Schumm 1991, 5). The view was not without merit; yet by the twentieth century, the incompleteness of the fossil record had become, in the words of Niles Eldredge and Stephen Jay Gould, “a catechism that brooks no analysis” (Eldredge and Gould 1972, 90). Moreover, it was a catechism whose repetition had injurious effects. No data set is complete—we speak of *the* human genome, but how many people living today have had their complete genomes sequenced? The answer is only a small number, and this level of incompleteness is typical of scientific datasets. Why then do paleontologists persist in self-flagellation over the incompleteness of the fossil record?

We saw in Section 2 that the leaders of the Paleobiological Revolutions had several responses to this state of affairs. One was to treat the fossil record as a reliable source of information about at least certain evolutionary patterns (Sepkoski’s “literal rereading”). So, for example, Gould wrote of the fossil record that it “is a faithful rendering of what [modern speciation] theory predicts, not a pitiful vestige of a once-

bountiful tale” (Gould 1980, 184). Others utilized large datasets to avoid some of the problems of local stratigraphic records, in particular, their incompleteness (“generalized rereading”). However, neither of these approaches proved to be a panacea. The former simply *asserted* that the fossil record can be relied upon, with little by way of stratigraphic principles to back this up; whereas the latter worked by abstracting away from field context to frame statistical generalizations, thus limiting its applicability. This paper has traced the emergence of a different approach to the fossil record, which works by analyzing the structure of the record and using this knowledge to inform sampling and interpretive strategies. A key benefit of this approach is its ability to dissect incompleteness and diagnose bias in the service of more reliable interpretations of the fossil record, including major biological events like mass extinctions (Holland and Patzkowsky 1999, 2012).

However, the approach has an ancillary benefit as well, and this is to call into question the entire discourse surrounding “incompleteness” and “bias” in paleontology. According to stratigraphic paleobiology, there are two sets of processes that serve as equal partners in the construction of the fossil record. On the one hand are biological processes including speciation, extinction, migration and community assembly. On the other are stratigraphic processes like sedimentation, erosion and condensation. The task of stratigraphic paleontology is to analyze how these sets of processes *interact* to produce the fossil record; it is not to understand how a “once-bountiful tale” is reduced to “a pitiful vestige” through stratigraphic filtering. The structure of the fossil record can then be regarded as a guide to sampling and analysis, not as the professional bugbear that it has too-long been (Holland 2017).

It is too early to know whether talk of the structure of the fossil record will replace talk of incompleteness and bias. However, if it does, it will be for reasons that the leaders of the Paleobiological Revolution would have appreciated. As Holland (2017) observes, “When we...write yet another paper about bias in the fossil record, that is what our colleagues hear. When they hear this repeatedly, they conclude that the fossil record is not worth bothering with” (1316). Gould had much the same worry. In his “Task for Paleobiology,” he chastises his colleagues for their “brutal pessimism” about the fossil record, not least because it implies a subordinate role for paleontology among the evolutionary sciences (Gould 1995, 1). Yet Gould had few arguments for why the fossil record could be trusted, beyond the suggestion that patterns in the fossil record seem to match certain expectations from evolutionary theory. Sequence stratigraphy places paleobiologists in a stronger position: by disclosing how the fossil record is physically assembled, it enables them to see the fossil record for what it is. And what it is is an unbelievably rich archive of the biological and environmental histories of our planet.

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Appendix to Chapter 5:

Some Important Stratigraphic Terms

Accommodation (or “accommodation space”): The vertical envelope between the sea surface and the basement of rocks beneath the sedimentary pile. Represents the space available for sedimentary accumulation.

Bedding plane: A surface that separates one layer of stratified rock from the preceding one.

Biostratigraphy: The branch of stratigraphy concerned with fossils and their use in dating and correlating strata.

Chronostratigraphy: The branch of stratigraphy concerned with the time-relations of strata, in particular, their relative time relations. (The branch of geology concerned with the absolute ages of strata and their contents is termed “geochronology.”)

Chronostratigraphically significant surface: A surface at which all overlying rocks are younger than all underlying rocks. If a surface is chronostratigraphically significant, it can be used as a temporal marker in local correlation.

Correlation: The activity of establishing a correspondence between strata, usually on the basis of their age (“temporal correlation”).

Craton: A large and ancient block of the earth’s crust that comprises the nucleus of a continent.

Depositional basin: A region of the earth’s surface where long-term subsidence creates accommodation space for the accumulation of sediments.

Depositional sequence: A relatively conformable (i.e., containing no major unconformities), genetically-related package of strata bounded by unconformities and their correlative conformities.

Diachronism: The phenomenon in which a sedimentary deposit or other geological unit varies in age with the place it was deposited.

Eustasy (or “eustatic sea-level change”): Global sea-level, which changes in response to the amount of water in the oceans and the volume of ocean basins.

Event stratigraphy: The stratigraphic study of short-lived events (on the scale of seconds to thousands of years).

Facies: A sedimentary deposit with a distinct set of physical, chemical and biological features, which permit its environment of origin to be inferred. From the Latin *faciēs* meaning “face” or “external appearance.”

Facies analysis: The activity that aims to identify aims to different environments in ancient rocks, and also to understand the range of processes that can operate within these environments.

First appearance datum (FAD): The first appearance of a fossil taxon at a location.

Flooding surface: A surface separating younger from older strata, across which there is evidence for an abrupt increase in water depth.

Highstand systems tract (HST): The third systems tract in a depositional sequence, characterized by “aggradational” (neither landward nor seaward) to seaward stacking patterns, and overlain by the sequence boundary.¹

Last appearance datum (LAD): The last appearance of a fossil taxon at a location.

Lithostratigraphy: The branch of stratigraphy concerned with the lithological (i.e., physical, as opposed to biological) features of strata.

Lowstand systems tract (LST): The first and lowest systems tract in a depositional sequence, characterized by seaward to aggradational (neither seaward nor landward) stacking, and overlain by the TST.

Maximum flooding surface: A flooding surface separating the underlying TST from the overlying HST. This surface represents the great water depth in a sequence and marks the turnaround from landward to seaward stacking.

Outcrop: A visible exposure of rock at the surface of the earth.

Parasequence: A relatively conformable (i.e., containing no major unconformities), genetically-related succession of strata bounded by flooding surfaces and their correlative surfaces. Internally, parasequences commonly display shallowing upward successions of facies (meaning that within a parasequence, shallow water facies lay atop deeper water facies). They are a major building block of depositional sequences.

Reflection seismology: A type of geophysical imaging technique used to image the subsurface part of the earth and to understand its geological features.

Sedimentary basin (see “Depositional basin”)

Sedimentology: The study of sedimentary rocks and the processes by which they are formed.

¹ This is an out-of-date definition of the HST, although for historical reasons it is the one employed in Chapter 5. In current practice, the HST is overlain by a fourth systems tract called the “falling stand systems tract” (FSST), which is overlain by the sequence boundary.

Seismology (see “*Reflection seismology*”)

Sequence (see “*Depositional sequence*”)

Sequence boundary: A surface that forms in response to relative falls in sea-level, and which often corresponds to a subaerial (i.e., formed in open air) unconformity.

Sequence stratigraphy: “The study of repetitive cycles of [sediment] accumulation followed by [gaps], at various time scales” (Miall 2015, 295), or “The study of genetically related facies within a framework of chronostratigraphically related surfaces” (Patzkowsky and Holland 2012, 220).

Stratigraphic paleobiology: “[The] intersection of sequence and event stratigraphy with paleobiology” (Patzkowsky and Holland 2012, 3).

Stratum: A rock layer or series of rock layers.

Systems tract: A set of contemporaneous depositional systems (packages of strata), defined by their positions within depositional sequences and by the stacking pattern of their constituent parasequences.

Unconformity: A surface of contact between two layers of unconformable (i.e., non-continuous) strata. Represents missing time in a stratigraphic succession, that is, a gap.

Tectonism: The deformation of the outer layer of the planet, which raises and lowers the earth’s crust (corresponding to the phenomena of “uplift” and “subsidence”).

Transgressive surface: A marine flooding surface separating the underlying LST from the overlying TST. Marks the point in a depositional sequence when seaward stacking is replaced by landward stacking.

Transgressive systems tract (TST): The second systems tract in a depositional sequence, characterized by the landward stacking of parasequences and overlain by the HST.

Zone: A stratigraphic interval characterized by the occurrence of a particular fossil assemblage, and usually named for a single, characteristic fossil taxon.